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ABSTRACT

This volume includes 12 of the 24 papers presented at the Third Annual California Linguistics Conference. Selections are drawn from each of the four sessions, covering semantic and lexical structure, phonology, syntax, and language in context. Each of the papers includes a bibliography, as well as diagrams, charts, and appendixes when necessary. (DD)

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STANFORD OCCASIONAL PAPERS IN LINGUISTICS

No. 3

Papers from the Third Annual California Linguistics Conference

Edited by

James Paul Gee
Carolyn Johnson
William R. Leben

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PREFACE

This volume contains 12 of the 24 papers presented at the Third Annual California Linguistics Conference at Stanford University on May 5th - 6th, 1973. The conference program is reproduced on the following two pages.

We are grateful to the authors for getting their corrected papers to us so rapidly after the conference and to the Committee on Linguistics at Stanford for permitting these papers to appear as an issue of Stanford Occasional Papers in Linguistics.

James Paul Gee
Carolyn Johnson
William R. Leben

THIRD ANNUAL CALIFORNIA LINGUISTIC CONFERENCE

May 5th - 6th, 1973
Stanford University

PROGRAM

SATURDAY, MAY 5

First Session: SEMANTIC AND LEXICAL STRUCTURE

1. Laurence R. Horn (USC): "Greek Grice: a brief survey of proto-conversational rules in the history of logic"
2. Donald Forman (UCSD): "Don't write that sincerity condition in the lexicon: can you hand me the pliers?"
3. Larry Gorbet (UCSD): "The isthmus of anaphor (idiomatically speaking)"
4. Susan Steele (UCSD): "Proximity breeds attachment: a reconsideration of Predicate Raising"
5. Mary Ann Campbell (CSU San Jose): "A fortuitous lacuna"
6. Leonard Talmy (Stanford Project on Language Universals): "Clause conflation"

Second Session: PHONOLOGY

1. Larry M. Hyman (USC): "The feature [grave] in phonological theory"
2. Robert Underhill (San Diego State): "On natural phonological classes"
3. Lawrence Johnson (USC): "Dissimilation as a natural process in phonology"
4. Asa De Matteo & Nancy Crawford (CSU San Jose): "On latent liquids"
5. Margaret M. Y. Sung (Stanford): "A study of literary and colloquial Amoy"
6. Alan Bell (U. of Colorado): "Elicitation and explanation of Lento forms"

SATURDAY EVENING, MAY 5

Morris Halle (M.I.T.): "Stress Rules in English: a new version"

SUNDAY, MAY 6

Third Session: SYNTAX

1. Harvey Rosenbaum (SWRL): "Constraints on extraction rules: the case from Zapotec"
2. Stanley E. Legum (SWRL): "On the necessity of semi-ordered rules"

~~3~~. Georgette Silva (UCLA): "On raising"

~~4~~. Alan Timberlake (UCLA): "The nominative as object in Finnish"

~~5~~. Robert S. Kirsner (UCLA): "Natural focus and agentive interpretation: on the semantics of Dutch expletive er"

~~6~~. Joseph Emonds (UCLA): "Constraints on base rules"

Fouth Session: LANGUAGE IN CONTEXT

~~1~~. Burckhard Mohr (CSC Dominguez Hills): "Linguistic variables and linguistic change in OHG texts of the 9th century"

~~2~~. Yorick Wilks (Stanford): "Preference semantics"

~~3~~. Ann Holmquist (Stanford): "Some unusual performatives in Whitman: a possible explanation"

~~4~~. Nancy Frishberg (UCSD) & Bonnie Gough (Salk Institute): "Time on our hands"

~~5~~. Robbin Battison (UCSD): "Phonology in American sign language: 3-D and DIGIT-vision"

~~6~~. Elaine Andersen & Carolyn Johnson (Stanford): "Modifications in a child's speech to younger children"

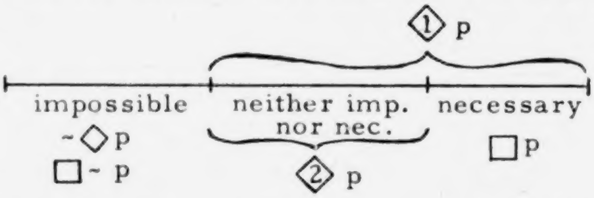
GREEK GRICE:
A BRIEF SURVEY OF PROTO-CONVERSATIONAL RULES IN THE HISTORY OF LOGIC

Laurence R. Horn
University of Southern California

This paper will constitute an attempt to sketch the treatment of certain modal and quantificational relations through the last couple of millenia, and to demonstrate that the correct analysis of these relations by conversational rules was in fact anticipated, if often unconsciously and inconsistently, by logicians from Aristotle to De Morgan. Our concentration will be focussed on the nature of the connection between the modal concepts possible and possible not and the quantificational concepts some and some not, and we shall begin with modalities.

De modalibus non gustabit asinus, declared medieval students of modal logic, 'it's an ass that doesn't have a taste for modals,' and -- as we shall see -- Aristotle was no ass, lacking that animal's stubborn consistency. In his use of necessary and impossible, Aristotle differs little from the generations of modal logicians he spawned, with the impossible equivalent to that which is necessarily not the case. But what of possibility? Aristotle found himself with two options, as a diagram will help make clear:

(1)



He could identify the possible as anything which is not impossible, including that which is necessary (◇① in the diagram). Or he could restrict possibility to that which is neither impossible nor necessary; that is, what today's logicians know as the contingent (◇②).

With these two options open to him, Aristotle — to the consternation of countless subsequent historians of logic -- chose both. In the Prior Analytics he stipulates, "I use the terms 'possibly' and 'the possible' of that which is not necessary, but, being assumed, results in nothing impossible." This is "two-sided possibility," distinguished from a one-sided variant admitted in the very next sentence: "We also say, ambiguously of the necessary that it is possible." The ambiguity here is that of possible, not necessary.

Aristotle points out that, given the bilateral definition, anything which is possible (to be) is also possible not (to be). In his terminology, all premisses in the mode of possibility are mutually convertible, via the principle of complementary conversion:

(2) $\Diamond p \longrightarrow \Diamond -p$

Thus, for example, 'it is possible that A belongs to B' may be converted (salva veritate) into 'it is possible that A does not belong to B.' Furthermore, given the symmetric nature of the conversion principle in (2), the latter may also be converted into the former. Indeed, as Aristotle notes, the symmetry of conversion assures the general equivalence of $\Diamond p$ and $\Diamond -p$.[1]

This principle is, in fact, often applied validly, especially in such contexts as those involving a raised personal subject and the ability sense of the modal, as in Aristotle's unexceptionable claim in his de Interpretatione that "everything capable of being cut or of walking is capable also of not walking or of not being cut" -- while the suppletive capable must be used in the English translation, Aristotle was able to employ the same term for possible here (dunaton) as he does in non-raised structures.

Having differentiated the two senses of possible, Aristotle proceeds to confuse them utterly. In particular, he systematically ignores the incompatibility of complementary conversion with the necessary $\supset$ possible entailment (de Int. 13, 22 b11). If that which is necessary is also possible and that which is possible is also possible not, then, by transitivity, what is necessarily true is possibly not true:

(3) i) $\Box p \supset \Diamond p$
ii) $\Diamond p \supset \Diamond -p$
iii) $\Box p \supset \Diamond -p$

It would then be possible that, e.g., $2 + 2 \neq 4$, or that $x \neq x$, surely as undesirable a consequence as ever haunted the nightmares of a modal logician.

What somehow escaped Aristotle's notice was the recognition that the entailment of (3i) applies to one-sided possibility alone (the two-sided sense being incompatible with necessity, as the chart in (1) indicates), while the law of complementary conversion in (3ii) is restricted to two-sided possibility. From

(3') i) $\Box p \supset \Diamond_{1} p$
ii) $\Diamond_{2} p \supset \Diamond_{2} -p$

nothing unwelcome can be inferred.

The pernicious, or perhaps, rather, careless ambiguity with which Aristotle wielded his possibility was resolved by his pupil Theophrastus, the "Old Peripatetic," but only at the cost of abandoning the insight expressed by the principle of complementary conversion and the identification of two-sided possibility as the normal sense of the term, the distinguishing characteristic of Aristotle's modal logic (Bochenski 1961, Kneale & Kneale 1961).

---

[1] For a sample of Aristotle's one-sided use of possibility, cf. de Int. 13, 22 a 14 ff. For the two-sided sense, see for example Pr. Anal. 13, 32 a 18 ff, 17, 37 a 15 ff. For additional discussion, cf. Bochenski (1947, 1961) and Kneale & Kneale (1961).

Theophrastus, in rejecting the conversion principle along with the bilateral definition of *possible*, struck the mold for virtually all modal logicians thenceforth, as he did in defining a secondary modal notion of *contingency*, applying -- like bilateral possibility -- to those propositions which are possible but not necessary:

(4) contigent (p) $=_{df} \langle 1 \rangle p \ \& \ \langle 1 \rangle -p$

The possible for Theophrastus is thus merely the not impossible (cf. *de Int.* 13, 22a14), and the *necessary* $\supset$ *possible* entailment can be, and is, retained.

A rare exception to the prevailing Theophrastian mold can be found in the *Dialectica* of Abelard, the outstanding lover of the scholastic tradition, who found time to identify *possibile* with *contingens* and establish a tri-valued modal logic based on *necessarium*, *possibile*, and *impossibile*. Unfortunately, his retention of the *necessary* $\supset$ *possible* entailment alongside the bilateral definition of possibility involved Abelard in the same inconsistency which had led to the coming of the Aristotelian modalities to grief.

By and large, however, Theophrastus' successors have so uniformly adopted the unilateral sense of possibility that many of Aristotle's translators have not hesitated to render only that one-sided sense as 'possible,' reserving 'contingent' for the two-sided notion -- this, despite the fact that, as noted above (and conceded by such perpetrators as Sir David Ross), Aristotle used the identical term for both!

Beyond the vale of logical possibility lies the epistemic notion -- *possible* as distinguished not from *necessary* but from *certain*. Epistemic modalities were not unknown to Frege, who considered *all* modal notions to involve a covert reference to knowledge and hence to be situated outside the realm of logic. It is not certain, however, that the logic of epistemic values (as formalized, e.g., by Hintikka, 1962) should not be considered as complementing, rather than replacing, the traditional notions of modality.

In any case, as von Wright insightfully points out (1951b: 32), while the epistemic *possible* occasionally corresponds to the one-sided logical possibility of Theophrastus -- the epistemically possible thereby being that which is merely as yet unfalsified -- it usually is taken to have the value *undecided*, corresponding to contingency or to the two-sided Aristotelian *possible*, signifying that which is *neither* known to be false *nor* known to be true.

The hoary principle of possibility, *ab esse ad posse* -- $p \supset \Diamond p$ -- is, von Wright suggests, invalid for the epistemic use of possible as undecided. He gives an example of a P.O.W. who might say "My mother is dead by now." up to, but not beyond, the point at which her death has been reported to him. Logicians who unqualifiedly accept the *ab esse ad posse* principle (which is, of course, a subpart of the *necessary* $\supset$ *possible* entailment) are, von Wright warns, "apt to ignore relevant facts about the use of language."

The key phrase here is *the use of language*, since the claim I would advance is that *both* logical and epistemic *possibles* are one-sided in *meaning* but two-sided in general *use*. This follows from the Gricean maxim of quantity (Grice, 1968): a speaker provides his auditor with all the information relevant to the context. If one knows something to be necessary or certain, one could hardly in good faith assert that it is merely possible. Like all such conversational implicatures, this one is subject to cancellation, although not without notice. Thus we may say that some proposition is (logically) possible if not necessary, or that it is (epistemically) possible, in fact certain. In the absence of qualification, however, possibility is taken to be upper-bounded by implicature, and hence bilateral.[2]

The same holds for the congruent notion of *permission* in von Wright's deontic logic (1951). Von Wright himself defines *permitted* unilaterally, like Theophrastus' *possible*, and retains the corresponding entailment: "what is obligatory is also permitted" (1951a: 4). Corresponding to the logically contingent (cf. (4)) and the epistemically undecided, von Wright defines the secondary, compound notion of moral indifference (not really quite as degenerate a value as it sounds):

(5) $\text{indiff.}(A) =_{df} \text{perm.}(A)\ \&\ \text{perm.}(-A)$

An act is (morally) indifferent if it is permitted and not obligatory.

By so constructing his deontic system of modalities, von Wright ends up in the unenviable position of defending the claim that "in the non-smoking compartment, not-smoking is permitted and smoking forbidden." (1951a: 6). But intuitively this statement, in attributing permissibility to that which is obligatory, strikes us as just as peculiar as 'It is possible that $2+2=4$,' 'It is possible that Earth is the third planet from its sun,' or — until recently -- 'It is possible for a Catholic priest to remain celibate.'

Von Wright, erstwhile champion of the language user, has shown that he too is quite capable of crucifying him, along with his intuition, on an altar of elegance, tradition, and logical symmetry.

The logical, epistemic, and deontic modalities are by no means the only values to which Grice's Maxim of Quantity pertains -- in fact, all predicates which fall on the weak end of an entailment scale share the property associated with *possible* and *permitted* of being infelicitously asserted when a stronger predicate is known to hold for the argument in question. Thus, just as 'it is necessary that *p*' entails 'it is (at least) possible that *p*,' 'it is hot' entails 'it is at least warm.' And it is correspondingly just as odd to describe a 95° day as (merely) warm as it would be to describe a necessary (or certain) truth as (only) possible.[2]

---

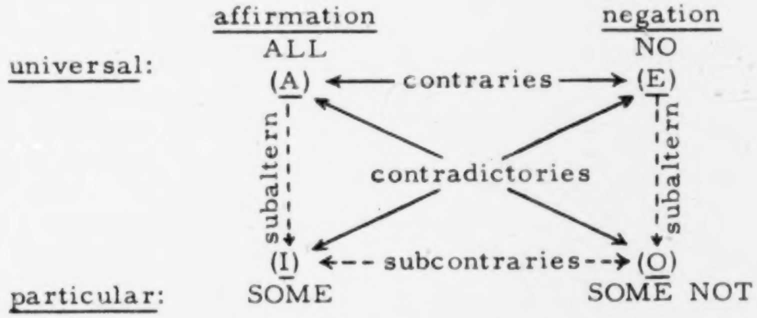
[2] Cf. Horn (1972) for an extended, if inconclusive, discussion of upper-bounding implicatures, cancellation and suspension, and scalar predication, and a more detailed examination of the relationship between the modals and quantifiers (including the binary connectives *and*, *or*, and *nor*) than will be given below.

In particular, the entailments and implicatures we have discussed for the modal values have direct analogues in the quantificational system. Nor is this surprising: we find at least as far back as Leibnitz the definition of the *necessary* as that which holds in *every* possible world, the *possible* in *some* possible world, and the *impossible* in *none*. This observation finds echoes in the modalities of Russell (1918: 231) and Carnap (1947: 136). The *contingent* would presumably hold in some worlds (e.g. the actual one) but not all, and it is this distinction between *at least some* and *some but not all*, or, as we might call them, one and two-sided *some* (respectively corresponding to classical possibility and contingency) which will now occupy our attention.

For the origin of the systematic treatment of quantificational notions, we must retrace our path back to our Greek Grice, for once again we become aware, as did Zeno Vendler (1967: 194) in a different connection, "that the path of inquiry we hoped to open is already marked with the footprints of Aristotle."

Aristotle's four quantificational forms and the oppositions obtaining among them can best be gleaned from the classical square of opposition in (6) below.

(6)



A = all S's are P
I = some S's are P
E = no S's are P
O = some S's are not P

The opposition between the universal affirmative A and universal negative E is one of contraries: both can be false simultaneously (in case the property is not "distributed" over the set S), but cannot be true. The oppositions along the diagonals between the universal negative and particular affirmative (E *vs.* I) and between the universal affirmative and particular negative (A *vs.* O) are contradictories: one term of a contradictory is true in any situation and the other false. On the other hand, Aristotle warns, the apparent opposition between the particular affirmative I and the particular negative O is only a verbal opposition and not a real one (*Pr. Anal.* Book II, Ch. 15). The terminology of a 'merely verbal opposition' is reminiscent of the treatment, elsewhere in the Prior Analytics, of the analogous modal terms *possible* and *possible not*.

While the *necessary* $\supset$ *possible* entailment of modal logic does not find its *all* $\supset$ *some* counterpart given explicitly in Aristotle's quantificational syllogistic, the so-called laws of subalternation indicated in (6) by the vertical dotted lines, relating the A and I vertices on the left side of the square and the E and O vertices on the right, are deducible in the Aristotelian system and were added to it by his commentators (cf. Bochenski, 1947, 1961).

The 'merely verbal opposition' between the particulars *some* and *some not* was also formalized by post-Aristotelians, in terms of subcontrariety (this opposition being literally *subcontrary*, under the contrary opposition). Just as at least one term of a contrary opposition must be false at any given time, at least one term of a *sub*contrary opposition must always be *true*. I and O can *both* be true, of course, in the event that neither A nor E is true.

It should be evident that we can simply superimpose the modal oppositions on the square in (6), with *necessary* fitting onto the A vertex, *possible* onto I (one-sided, that is), *impossible* onto E, and *possible not* onto O. *Necessary* is thus the contrary of *impossible* but the contradictory of *possible not*, and so on. Notice that the subcontrary opposition, and likewise the subaltern entailments, are applicable only for *one*-sided *possible* and *one*-sided *some*, *at least some* rather than *some but not all*.

The loose thread binding the quantificational subcontraries was picked up two thousand years later by Sir William Hamilton of Edinburgh who finds, with Aristotle, that the purported subcontrary opposition between, for example, 'Some men are learned.' and 'Some men are not learned.' is an opposition in language rather than in reality, one which Hamilton claims to have been "only laid down from a love of symmetry, in order to make out the opposition of all the corners in the square of opposition" (1860: I. 261).

Hamilton, however, departs from the Stagyrite and virtually everyone else in establishing the bilateral *some* as the basic sense of the particular. He declares, in words which curiously foreshadow the Gricean postulates of a century thence, that "*some*, if not otherwise qualified, means *some only*-- this by presumption" (II. 254). The contextual nature of this presumption is pointed out when we consider its cancellation: "we ought to be able to say *some at least* when we do not know, and cannot, therefore, say determinately either that *some only* or that *all* is true" (ibid.).

Ignoring his own reminder that the presumption of bilaterality for *some* is a matter of context and qualification, Hamilton seeks to elevate it to the status of logical representation; while defining both unilateral and bilateral concepts of the particular (which he designates as *indefinite* and *semi-definite*, respectively), he opts for the latter, just as did Aristotle in the parallel case for the modal *possible*.

Hamilton notes that the *all* $\supset$ *some* entailment holds only if *some* is taken as *not none*, *some at least*; "for if we think by the *some*, *some only*, that is, *some* exclusive of *all*, so far from there being any competent inference, there is in fact a real opposition" (II. 267). *All* and (semi-definite) *some* are, in Hamilton's happy phrase, "incompossible."

Unfortunately for Sir William, he was not the clearest or most consistent logician who ever posited, and he left himself fatally open to attack by most of his contemporaries and successors. The debates raging between his camp and that of Augustus De Morgan in the mid-nineteenth century over the nature of some bear more similarities in style to the forensics of the schism within generative grammar during the last several years than to the more germane but more civilized differences among Aristotle and his commentators as to the value of possible.

De Morgan demonstrates that Hamilton, despite his redefinition of some as only some, an intermediate value between none and all, appears to base his syllogisms on the older, traditional sense some at least, not incompossible with all. The bilateral, innovative usage surfaces only in Hamilton's immediate inferences, those deductive inferences dealing with one proposition alone, considered in isolation. There is, here, a direct parallel with Aristotle's use of unilateral possible in his modal syllogistic, alongside the bilateral possible apparently restricted to immediate inferences via complementary conversion.

The incoherence of Hamilton's system of quantifiers De Morgan attributes to the Scotsman's "strange hatred of the mathematical sciences," which in turn derives from his "unusual inaptitude for them, which prevented any real understanding, even of their lowest branches." These remarks emerge from De Morgan's article on logic for the English Cyclopaedia's 1860 edition, an entry otherwise rather generous to Hamilton.

De Morgan elsewhere excuses a "hurried article" in which Hamilton seems to be claiming that the Aristotelian some includes possibly none, on the grounds that Hamilton had to publish his findings quickly "out of fear that mathematics would destroy his fabric, unless he could first destroy mathematics." Hamilton, it should be added, got his licks in too, but less effectively, having some time earlier accused De Morgan simultaneously of plagiarism and linguistic counter-intuitiveness, another procedure not without parallel in our own day.

It should not be thought that just because Hamiltons' logic was natural but inconsistent, De Morgan's must necessarily have been consistent but unnatural. Indeed, invective aside, it was De Morgan who, even more directly than Hamilton or Aristotle, prefigured recent work on the pragmatics of logical operators.

"Common language," De Morgan points out, "makes a certain conventional approach to definiteness which has been thrown away in works of logic," by which approach some usually means a small fraction of the whole, many a large fraction, and most somewhat more than half, with nearly all designating a larger fraction still. Not that this discarding of conventional usage is a mistake -- De Morgan insists on the retention of the logician's some with an indefinite upper bound, and retains the subaltern inference of some from all (De Morgan, 1847:100).

De Morgan's arguments for the unilateral *some* ultimately held sway over such logicians of the next generation as Jevons, Spalding, Veitch, and Keynes. As Keynes argues in his 1906 *Logic*, the *some at least* sense is more basic than the *some only* in that the former implies the latter, while the latter can be regarded (like modal contingency in (4) and deontic indifference in (5)) as a compound of two propositions of the I and O forms. Furthermore, given a situation (like that cited by Hamilton above) in which the speaker's knowledge is incomplete -- if all the S's he knows are P -- he can't use either 'all S's are P' or -- with Hamilton's bilateral *(only) some* -- 'some S's are P' : "the only solution . . . is to say that all or some S's are P's. The complexity that this would introduce is obvious." (Keynes 1906: 202-3).

Kenyes neglects to add that speakers who find themselves in such a position have frequent recourse to just such an obvious complexity in order to banish any lingering implicatures: "Some or all of my friends will be there," "Some if not all men are irrational animals." and so on.

Despite his staunch unilateralist position, Keynes is willing to concede that many logicians "have not recognized the pitfalls surrounding the use of *some*. Many passages might be quoted in which they distinctly adopt the meaning -- *some, but not all*" (p. 200).

To which Otto Jespersen retorts in his *Philosophy of Grammar*, "in the name of common sense, . . . why do logicians dig such pitfalls for their fellow-logicians to tumble into by using ordinary words in abnormal meanings?" (1924: 324). Like his forbears Aristotle, Hamilton, and the pitfallen logicians chastised by Keynes, Jespersen fails to recognize that meaning, in the broad communicative framework, is composed not only of the rules of logical form -- meaning proper -- but also of the rules of pragmatics which determine use of a term or utterance in context. We can rescue the baby of Aristotelian and Hamiltonian insight into the function of logical operators in ordinary language from the bathwater of logical inconsistency by accepting the logical status of the subaltern inference (from *all* to *some* and from *necessary* to *possible*), while "demoting" the subcontrary conversions (from *some* to *some not* and *possible* to *possible not*) to the role of contextually-determined Gricean rules of implicature.

The oddness of the assertion that "Some men are mammals." together with the systematic "ambiguity" of *some*, *possible*, and weak scalar values in general, would thus receive a pragmatic, rather than strictly semantic, explanation.[3]

---

[3] I do not mean to imply (or presume) here that the division between semantics and pragmatics is easy or straightforward to define, nor indeed that pragmatic considerations play no role in determining semantic and syntactic form. On the contrary, there is every reason to believe that the interactions between a speaker's pragmatic competence and the rest of his linguistic system are even more intimate than is obvious from various recent papers of George Lakoff and Robin Lakoff, e.g. Gordon & Lakoff, 1971, from the conversational constraints on lexicalization formulated in Horn, 1972 (Chapter 4).

As a final illustration of the anticipation of conversational rules within the history of logic, we shall touch upon the matter of fallacies. As Geis & Zwicky mention in their squib on invited inferences, such traditional fallacies as *denial of the antecedent* (cf. *affirmation of the consequent*) and *post hoc ergo propter hoc* have their analogues in pragmatic rules, although I would maintain, as they do not, that these rules are Gricean in nature, deriving from the Maxim of Relevance.

In their celebrated example, Geis & Zwicky (1971) point out that most non-logicians, on being told "If you mow the lawn, I'll give you $5." infer that if they *didn't* mow, they *wouldn't* get the money, despite the presumably unjustified nature of the inference from *if P then Q* to *if -P then -Q* and the consequent fallacy they are committing by accepting the "invitation" of this inference. The inference, however, is natural enough: by stipulating P as a *sufficient* condition for Q, we implicitly suggest that P is a *necessary* condition as well -- else, why mention it?

Another beloved fallacy dating back to Aristotle is the *post hoc ergo propter hoc* or false cause argument, involving the inference of a causal relation from a temporal sequence, as in the attribution of causality in the Geis-Zwicky example, "Having finished the manuscript, she collapsed in a swoon." The natural tendency of speakers of non-formal languages to partake of this fallacy is revealed by their predilection for temporal metaphors to indicate causal relationships -- in English we have Q *follows* from P; Q *since* P; if P *then* Q; Q is a *consequent* of P; even if P, *still* Q; and doubtless many other cases in which the temporal metaphor is beyond raising from the dead.

A recent example of such a transference is found in a headline announcing *Lebanese Prime Minister Resigns in Wake of Israeli Commando Raids*, where the causal connection is clearly implied without being asserted.

One more venerable fallacy is that of *secundum quid*, confusing what is predicated of part with what is predicated of the whole, as in the presumably fallacious inference from 'the flag is red' to 'the flag is all red.' But once more, in the absence of further qualification, a '*red* flag' *would* be assumed to be *all* red in a normal conversational context. The American or French flags could not be described as red (*tout court*) without hopelessly (or diabolically) misleading one's auditors. The Maxim of Quantity strikes again!

As a final example, we can take the logical dictum that nothing can be inferred in a syllogism when both premisses are negative.[4] A beautiful counterexample to this observation, at least as far as ordinary usage is concerned, is provided by De Morgan (1864: 277ff). In the course of explaining the relationship between *some* and *not all*, De Morgan points out that "the common usage of mankind inclines to partition: even the affirmation of *none* is, whenever it can be so made, affirmation of *some* of the alternative kind."

---

[4] Cf. Hamblin (1970) for a detailed and enthralling account of these and other favorite fallacies of yesterday and today.

As evidence, De Morgan cites the following:

> Jack Sprat could eat no fat,
> His wife could eat no lean,
> And so, betwixt them both,
> They licked the platter clean.

The implicature is revealed by the so in the third line, and even more clearly by the widely inextant second stanza (emphasis and parentheses De Morgan's):

> For (i.e. as implied) Jack ate all the lean,
> And Joan ate ate all the fat,
> The bone they picked quite clean,
> And gave it to the cat.

As De Morgan concludes, "two abstinences make complete consumption."

A moral leaps forth to be drawn from this discussion, and it can perhaps be best expressed in the form of an answer to a rhetorical question posed by Hamblin in his valuable book on fallacies (1970: 36): "Every invalid inference schema ... could, in theory, be dignified with a special name and treated similarly [i.e. to the classical fallacies], yet we do not hear of any others. Why not ..., say, the Fallacy of Distributing Quantifiers without regard to Negation Signs?"

Why not is, of course, because of the absence of any pragmatic rule of conversation which would make such an inference likely. We are only warned against those sins we are considered susceptible of commiting.

Just as the prescriptivist linguist provides us with historical evidence of the way people actually spoke by warning their contemporaries against it, so too the prescriptivist logician, by banishing specific forms of argument, reveals to us how people actually reason.

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HOW TO ASK QUESTIONS AND INFLUENCE PEOPLE*

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Austin (1955) has shown that virtually every sentence has associated with it what he calls an illocutionary force; that is, that it is used to perform speech acts such as asserting, ordering, pronouncing man and wife, etc. Grice (1967) has shown that illocutionary forces are not simple to compute; in particular, a sentence can often be used to convey an implicature in addition to or instead of its literal meaning. The sociologists Sacks and Schegloff, working in a very different framework, have investigated the complex sequential constraints among the illocutionary forces of utterances in conversation. Taken together, these investigations suggest a broad area of research which I call conversational analysis. Conversational analysis overlaps linguistics, sociology and philosophy. It might be defined in terms of the two main interrelated questions which it asks:

(1) a. How can one identify the illocutionary force of an utterance?
    b. How do illocutionary forces affect the sequencing of utterances in a discourse?

Conversational analysis overlaps with the traditional realm of discourse analysis, but it emphasizes formal features of conversation rather than content.

This paper is concerned with a small problem in conversational analysis, a subproblem of question (1a.), namely how certain apparent questions and assertions are often used to convey certain other speech acts such as requests. Gordon and Lakoff (1971) have laid the basic groundwork for a conversational solution to this problem. My object here is to show that by refining their analysis one can account for a wide range of speech acts. In doing so I hope to add to the evidence that conversational problems are closely bound to more traditional problems in syntax and semantics.

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There are undoubtedly lots of mistakes and errors in this draft, and as usual they are solely my responsibility. I hope readers will notify me of some of them which they may find.

1. How Gordon and Lakoff get people to take out the garbage.

Gordon and Lakoff (1971) are concerned with the sentences:

(2) a. I want you to take out the garbage.
  b. Can you take out the garbage?
  c. Would you be willing to take out the garbage?
  d. Will you take out the garbage?

The first of these looks like an ordinary declarative, and the others look like questions; yet all of these sentences can be used to convey requests.

To account for (2), they investigate the preconditions for making a sincere request and propose the following set of sincerity conditions:

(3) a. SINCERE (a, REQUEST (a, b, Q)) → WANT (a, Q)
  b. SINCERE (a, REQUEST (a, b, Q)) → ASSUME (a, CAN (b, Q))
  c. SINCERE (a, REQUEST (a, b, Q)) → ASSUME (a, WILLING (b, Q))
  d. SINCERE (a, REQUEST (a, b, Q)) → ASSUME (a, -Q)

  where Q is of the form FUT (DO (b, R)) [b will do act R]

"Thus, if a sincerely requests of b that b do R, then a wants b to do R, a assumes that b can do R, a assumes that b would be willing to do R, and a assumes that b will not do R in the absence of the request." There is little argumentation for the content of the individual conditions. They are simply cited as theorems derivable from a meaning postulate which characterizes the notion of sincerity. Gordon and Lakoff note that they gloss over the "in the absence of a request" in the interpretation of d. They do not note that the translation of c. has been put in the subjunctive and is somewhat awkward if rephrased indicatively. Presumably they rely on the reader's intuition to support the conditions, and indeed at first sight they seem reasonable.

Having stated these conditions, Gordon and Lakoff note a correlation between the sentences of (2) and the conditions of (3). If one calls (3a.) a speaker based sincerity condition and (3b.-d.) hearer-based sincerity conditions, they propose:

(4) One can convey a request by (i) asserting a speaker-based sincerity condition or (ii) questioning a hearer-based sincerity condition.

A request can be conveyed, however, only if the hearer assumes that the speaker does not intend to convey the question. Thus, they derive the two different uses of each sentence of (2) from the same underlying structure.

It seems to me that Gordon and Lakoff's basic idea is a correct and an important one, for it captures the facts about these sentences which to all appearances would be difficult to handle in purely transformational terms. In the following section, however, I shall argue for modifications in their statements of both (3) and (4).

2. Telling, requesting, asking -- more garbage

To see some of the limitations of Gordon and Lakoff's solution, lets expand their paradigm somewhat.

(5) a. I want you to take out the garbage.
a'. *I would want you to take out the garbage.

b. Can you take out the garbage?
b'. Could you take out the garbage?

c. *Will you be willing to take out the garbage?
c'. Would you be willing to take out the garbage?

d. Will you take out the garbage?
d'. Would you take the garbage?

(The starred sentences are not ungrammatical, but lack the relevant reading.) The problem is that some of the subjunctive forms pattern like indicatives, but others don't.

Also compare the primed sentences with the unprimed in terms of the tone they express. (5d.) a parent might say to a child, but would I use it if I were asking a favor of a friend and peer, say of one of my roommates? I think not; then I would prefer (5d'.). Similarly, the difference between (5b.) and (5b'.) is not overwhelming, but the subjunctive form is a trifle more courteous. The indicative sentences are somewhat stronger then the subjunctive ones; they are almost orders.

In fact, the situations in which the indicatives are appropriate correspond closely to the range of use of the verb "tell" as in

(6) A told B to do X.

Accordingly, I shall call such speech acts "tellings." The subjunctives I shall go on calling requests.

Now Heringer (1971) explains the differences between tellings and requests by claiming that putting a request in the subjunctive emphasizes its conditionality and thus renders it more deferential. This solution, though, does not account for the starred sentences of (5), nor does it relate this case to any sort of underlying semantics for the subjunctive. It gives no rules for when the speaker should choose the more or less polite form. Some versions of polite subjunctivism might also have trouble with

(7) I would like you to take out the garbage.

which is too imperious for a request; it is a telling.

To find a better explanation, let's examine in more detail the sincerity conditions for telling and requesting. Surely, it is normal for a teller to want the telling to be obeyed. In fact, there is no question of willingness: a defining condition for a sincere act of telling is the speaker's belief that he has some authority, so that he will be obeyed regardless of the tellee's willingness. Notice that such social constraint justifies him in assuming that the request will be granted, even though he can't have definite knowledge (since social norms can be violated). But this expectation of being obeyed places a constraint on the teller: it is insincere if not downright cruel to tell someone to do something unless one believes that the tellee is able to do it. We might formalize the set of sincerity conditions:

(8) a. SINCERE (a, TELL (a,b,Q)) → WANT (a,Q)
b. SINCERE (a, TELL (a,b,Q)) → ASSUME (a, CAN (b,Q))
c. no condition on willingness
d. SINCERE (a, TELL (a,b,Q)) → ASSUME (a,Q)

where Q is of the form FUT (DO (b,R)).

The first two conditions are just like Gordon and Lakoff's. (8d.) is the teller's assumption that the addressee will perform the specified act if asked; it eliminates the somewhat suspicious negative from (3d.), but it does still have a conditional element that I don't know a good way of formalizing. Notice that (3d.) is really too strong, anyway, for it is perfectly appropriate to issue a telling or request when one doesn't know whether the addressee would carry it out if not asked. If we assume a general enough interpretation of (4) to include tellings, we find that (8) yields precisely the unstarred indicative sentences of (5).

Now, what about requests? The difference between telling and requesting is that the requester doesn't necessarily have grounds to assume that his request will be complied with. Thus,

(9) d. Would you babysit for me Friday night?

is an acceptable request so long as the requester can assume that there is a reasonable chance of compliance. As a result, willingness, which was irrelevant for tellings, becomes a factor. Condition (3c.) is too strong, though, for one can ask:

(9) c. Would you be willing to babysit Friday night?

when one does not know whether the requestee will be willing or not. Rather, it is only necessary for the requester to assume that the requestee be likely to be willing. Similarly, one doesn't always know whether the requestee can comply; one might in all sincerity ask:

(9) b. Could you babysit for me Friday night?

without knowing whether the requestee actually could. The sincerity condition is that the requester must assume that there is a reasonable chance that the requestee can. Thus, instead of (3),

(10) a. ?
b. SINCERE (a, REQUEST (a,b,Q)) → ASSUME (a, LIKELY (CAN (b,Q)))
c. SINCERE (a, REQUEST (a,b,Q)) → ASSUME (a, LIKELY (WILLING (b,Q)))
d. SINCERE (a, REQUEST (a,b,Q)) → ASSUME (a, LIKELY (Q))

The predicate LIKELY here is only an approximation — perhaps a better translation would run something like "there is a reasonable chance that" -- but it does seem that there is one sort of modality operating for all three conditions.

Ignoring for the moment the obvious gap in (10), how could we use (10) to derive requests? If we interpret (4) broadly again, the most obvious ones are?

(11) $$\left\{\begin{array}{l}\left\{\begin{array}{l}\text{Are you}\\ \text{Would you be}\end{array}\right\}\text{ likely to}\\ \text{Is there a reasonable chance that you}\left\{\begin{array}{l}\text{will}\\ \text{would}\end{array}\right\}\end{array}\right\}\left\{\begin{array}{l}\text{be able to}\\ \text{be willing}\\ ??\emptyset\end{array}\right\}\text{to babysit}$$

(for me) Friday night?

These can serve as requests, but they have a very deferential tone. I don't know how to interpret the subtle gradations, but one possible cause is the lack of an exact surface gloss for LIKELY.

But suppose LIKELY could be realized on the surface as the subjunctive mood. Then we could derive the remaining good sentences of (5). I have no strong independent argument to prove this equivalence. However, semantically it is not at all implausible. The subjunctive mood is associated in lots of languages with uncertainty. Consider the use of "would" in non-conditional independent clauses in English:

(12) Where can we find the answer?
John would know it.

It seems to me that (12) involves a slightly higher degree of likelihood than the sincerity conditions; therefore I can not claim directly that the same element is involved. However, the two uses of "would" have the common element that the speaker can not really have access to the information being claimed without consulting the person being referred to. Thus, LIKELY can be seen as a hedge used to qualify assertions about pragmatically privileged information, and can underlie the subjunctives of (5).

Now consider:

(13) a. ??I want you to hand me that pencil.
b. Can you hand me that pencil?
c. $*\left\{\begin{array}{l}\text{Will}\\ \text{Would}\end{array}\right\}$ you be willing to hand me that pencil?
d. Will you hand me that pencil?

This paradigm looks like a hybrid of requests and tellings. When would one use it? Suppose, for example, that the pencil is located in the addressee's reach so that he could conveniently hand it to the speaker without getting up. In such a situation the addressee is socially constrained to accede, and failure to do so would be a violation in a way that is not true of the requests of (5), where saying no is an ordinary response. Thus as in telling, willingness is not in question, but the addressee can assume compliance. Then he must also assume ability. In other words, the sincerity conditions would be no willingness condition. "Tell" is too strong a verb to describe this speech act, yet "request" is somehow too stiff:

(14) ? I requested for John to hand me the pencil.

Accordingly, I shall use ASK, though sentences like

(15) I asked Quetzlcoatl to take out the garbage.

suggest that "ask" is too general a verb for the purpose.

(16) a. ?
b. SINCERE (a, ASK (a, b, Q)) → ASSUME (a, CAN (b, Q))
c. no condition on willingness
d. SINCERE (a, ASK (a, b, Q)) → ASSUME (a, Q)

Given a broad interpretation of b., these conditions predict the sentences of (13).

By now the attentive reader will have noticed that I haven't said much about the a. conditions for the different types of requests. In fact, I don't have any fully satisfactory analysis for them. At first glance it would be intuitively satisfactory to retain Gordon and Lakoff's condition (3a.). Unfortunately, I don't find "I want" sentences adequate for either asking or requesting. At least when the wind is west by northwest, though, I can get

(17) a. I wish you'd take out the garbage.
b. I wish you'd hand me that pencil.

as requesting and asking. One might then propose as sincerity conditions:

(18) a. SINCERE (a, REQUEST (a, b, Q)) → WISH (a, Q)
b. SINCERE (a, ASK (a, b, Q)) → WISH (a, Q)

but I have no semantic explanation why they should work. In some sense "wish" feels weaker to me than "want," but I don't understand the distinction well enough to use it to justify (18). "Would like," which is clearly an idiom, patterns like "want" to form tellings:

(7) I would like you to take out the garbage.

One final problem arises in the actual use of the sentences under discussion. Without any good data to go on, I suspect that request-forms are often used instead of tellings, and vice-versa. The first case is not surprising. Someone with the authority to tell may still choose to be more courteous and to request instead; he violates no sincerity condition. The reverse case would be harder to account for; perhaps it would tend to correlate with a pushy personality. I suspect in any case that even if such violations occur, one could write a variable rule which would show the more polite forms used more often in more polite situations. I hope to gather some empirical data on this problem.

3. A further CAN of worms (if available)

So far I have been blithely ignoring the fact that "can" has at least four different meanings: a logical sense and an epistemic sense indicating different types of possibility, a deontic use indicating permission, and an ability use.

(19) a. It can/could be raining in Chicago. (logical or epistemic)
     b. Can/could/may I please go to the movies. (deontic)
     c. I can lift a hundred pounds with my bare fingers. (ability)

But of course the CAN mentioned throughout this paper has obviously been the ability can; the others don't make any sense in the relevant contexts.[1]

Before taking this obvious wisdom too seriously, though, let's examine

(20) a. I can't talk to you right now.

(20) seems to be ambiguous. If written on the blackboard by an obviously silent professor, it might suggest that he has laryngitis, and lacks the capability to talk. But what about when someone utters this sentence out loud? The capability reading then becomes paradoxical. Rather the sentence means something like "I don't have the time to talk to you right now," or "I'm not available to talk to you right now." A similar example with "able":

(20) b. Mr. Houdini is too tied-up to be able to talk to you right now.

I can't give a precise semantic characterization for this reading of "can," but I shall refer to it as availability.

This type of ambiguity seems to hold for a wide range of sentences, and the restrictions on it are fairly complex. Present tense "can" sentences without time adverbials are fairly free in their ambiguity, but time adverbials virtually force an availability reading:

(21) a. John can swim the English Channel. (availability or capability)
     b. John can swim the English Channel $\left\{\begin{array}{l}\text{today.}\\ \text{this Friday.}\\ \text{next year.}\end{array}\right.$ (availability)

Past tense sentences with "could" are ambiguous, but the availability reading is reinforced by a point-in-time adverbial like "last Friday," whereas the capability reading is favored by a long-duration time adverbial such as "last year." Thus, both of the following are ambiguous:

(22) a. No one could swim the English Channel last Friday.
     b. No one could swim the English Channel last year.

but it is much easier to get an availability reading on (22a.) and a capability reading on (22b.). Sentences in all tenses with "able," in contrast, seem freely ambiguous between the readings:

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[1] The reader concerned with the details of these distinctions should see section 2.2 of Horn, 1972.

(23) a. John is able to swim the English Channel today. (either reading)
b. Fred will be able to swim the English Channel this Friday. (either reading)
c. Mark wasn't able to swim the English Channel last Friday. (either reading)

"Capable" does not display the availability reading at all:

(24) John is capable of swimming the English Channel right now. (capability)

The difference between "can" and "able" in the present tense seems especially odd, but I have no explanation for these facts.

The traditional ability reading of "can" I am now calling "capability." But which reading is right for sincerity conditions? Let us begin by assuming the traditional capability one. Notice that an adequate natural logic must contain the theorems:

(25) a. FUT (DO (b, R)) → CAN (b, DO (b, R))
b. LIKELY (FUT (DO (b, R))) → LIKELY (CAN (b, DO (b, R)))

But notice then that a sincerity condition stating that the speaker assumes that the addressee will comply with his request has as a corollary that the speaker assumes that the addressee can comply with his request. In other words, the sincerity conditions on ability, if interpreted in terms of capability, are redundant and only justified by the apparent need to derive requestions.

At first glance the same argument seems to apply to availability "can." Thus

(26) ? ? I can't babysit this Friday even though I will.

is extremely awkward. But by putting in the right context, it can be immensely improved:

(27) I can't babysit for Marcie this Friday because of my board meeting, but knowing how she twists me around her finger, I probably will (end up doing it for her) anyway.

Thus, unavailability and futurity are not incompatible, and (25) is not a theorem for the availability sense of CAN. In all the sincerity conditions the availability interpretation makes at least as much sense as the capability one. Thus, conditions are needed using availability CAN, and there is no evidence for additional ones involving capability CAN. We must assume that availability is the relevant reading.

4. Grabbing sincerity conditions by the entailments

There are certain requests which require a principle in addition to (4). First let's look at the requestions:

(28) $\begin{array}{l}\text{a.}\\ \text{a}'.\end{array}$ $\left\{\begin{array}{l}\text{Are you}\\ \text{Would you be}\end{array}\right\}$ tall enough to hang this picture for me?

$\begin{array}{l}\text{b.}\\ \text{b}'.\end{array}$ $\left\{\begin{array}{l}\text{Do}\\ \text{Would}\end{array}\right\}$ you have enough time to answer a few simple questions?

These could be paraphrased by:

(29) a. $\left\{\begin{array}{l}\text{Are you}\\ \text{Would you be}\end{array}\right\}$ tall enough to be able to hang this picture for me?

b. $\left\{\begin{array}{l}\text{Would you}\\ \text{Do you}\end{array}\right\}$ have enough time to be able to answer a few questions?

These are not accounted for by any of the sincerity conditions discussed above, and furthermore they have much more of a question tone than the other sentences.

An intriguing possibility is that "enough" involves "can" in its semantic structure, for they seem to share ambiguities. (29a.) and (29b.) give capability and availability readings, and (30a.) and (30b.) illustrate epistemic and doentic ones:

(30) a. This new evidence is incriminating enough for (it to be possible for) Sacco to have been guilty after all.

b. Have you been a good enough boy (to be allowed) to go to the movies?

Horn (1972) gives several other parallels between "enough" and "can," and discusses their relationship.

There are enough problems with deriving "enough" from "can," though, that I wish for now only to claim that "can" sentences entail "enough" ones. Thus

(31) You can hang this picture. (capability)

entails

(32) You are tall enough to hang this picture. (capability)

and in fact entails

(33) You are X enough to hang this picture. (capability)

for all relevant X's. Similarly,

(34) I can see John. (availability)

entails

(35) I have enough time to see John. (availability)

The requestness of (28) cannot be accounted for by the conditions I have described above. But suppose we add

(36) One can convey a request by questioning the entailment of a hearer-based sincerity condition; furthermore, this type of sentence retains much more of a question quality than the direct questioning of a sincerity condition itself, and the request is conditional on an affirmative answer to the question.

Then from (25), (36), and the entailment between (31) and (32), we can predict that (28a.) conveys telling someone to hang the picture. Similarly, (28a'.) conveys a parallel request. Notice, by the way, that (36) at least partly contradicts the notion that the conveying of a request excludes the conveying of a question by the same utterance.

Now look at

(37) a. (Excuse me,) can you tell me which is the train for Flatbush?

I could walk up to someone waiting on a subway platform and ask this, even though I'd have no way of knowing for sure whether or not they would know the answer, and hence whether they would have the capability to answer. But compare:

(37) b. (Excuse me,) can you please tell me which is the train for Flatbush?

Everyone who I have asked has agreed that the following distinction is subtle, but they have all agreed that it is real for them. The only change from (37a.) to (37b.) is the insertion of the word "please," yet (37b.) would be appropriate only if addressed to an employee in a ticket booth or some similar personnel; I would not address it to an arbitrary stranger in the crowd. How can this difference be?

(37b.), or (37a.) if addressed to a ticketseller, is a typical example of asking, and meets all the relevant conditions; in particular, social norms would require an answer. But consider (37a.) as addressed to an arbitrary stander-by. The conditions for asking seem not to be met. In particular, since one doesn't know whether the askee knows the information, one can not assume that they will give it. But this is the crucial characteristic of an asking based on (36): the request will be conditional on the answer to the question used to convey it. Thus, if the askee does know the information, he is socially obligated to give it. And in this situation (37a.) really does feel like a question as to ability in addition to its requestiness. From this we can see why "please" can not occur in this situation. "Please" occurs only in requests, and it appears that requestions formed by (36) are too questiony to contain "please." All that remains is to show that (36) really can predict (37a.). Consider the futurity condition for an asking. It entails an ability condition. Thus, we can see (37a.) as questioning that entailment of a sincerity condition, and (36) predicts it.

5. Whereto from here?

There are lots more speech acts that can be conveyed by questions and statements. Heringer (1971) states general sincerity conditions for classes of speech acts including assertions, questions, granting permissions, promises, offers, commands, and requests. Since he does not make use of the distinctions which I have been discussing, his principles can not be preserved as stated. Unfortunately, I have just become aware of his work since writing most of this paper, and I am not yet ready to integrate my conclusions with his. There do seem, however, to be general principles to predict sincerity conditions across broad classes of speech acts, and I hope to investigate these in the near future. Also, the similarities between conditions (4) and (36) are too striking to be ignored. They should very likely be collapsed into one formulation.

I hope I have shown that there are some intriguing problems in conversation waiting for solutions.

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THE ISTHMUS OF ANAPHOR (AND IDIOMATICITY)[*]

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In support of his claim that idioms are unanalyzable, Fraser (1970) asserts that various anaphoric processes — pronominalization, gapping, and conjunction reduction — do not apply across idiom boundaries; i.e., that idioms are anaphoric islands in the sense of Postal (1969). For example, the following anaphora is not permitted:

(1) *Having to pay through the nose for inedible food really puts mine / it out of joint.

Fraser further posits a "Frozenness Hierarchy"[1] — a hierarchy of unique levels to which any idiom may be assigned on the basis of the types of transformations it does or does not undergo. Specifically, undergoing the transformations of one level implies susceptibility to all those of lower (i.e., more frozen) levels whose structural description is met; and failure to undergo a transformation at one level implies failure at all higher levels.

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I offer, however, what should be the universal warning to accompany papers on idioms: they vary a lot idiolectally, so be not dismayed if some of my judgments are disagreeable. I try to give enough examples at crucial points that some examples will be convincing to most linguist/speakers. To save needless mindwarp, before getting upset over something horrible you can do to an idiom, check that the same process does not look just as bad with similar non-idiomatic material.

[1] Fraser's Frozenness Hierarchy (1970)

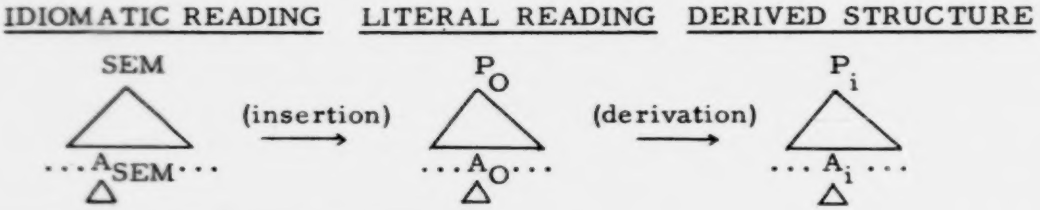
[L6] UNRESTRICTED
[L5] RECONSTITUTION (e.g. Action Nominalization)
[L4] EXTRACTION (moves material out of idiom)
[L3] PERMUTATION (permutes elements within an idiom)
[L2] INSERTION (moves material into an idiom)
[L1] ADJUNCTION (attaches affixes — e.g. Gerundive Nominalization)
[L∅] COMPLETELY FROZEN ("no operation whatsoever may apply")

In this paper I will show that anaphoric processes do indeed apply across idiom boundaries, that they apply in a normal and quite regular fashion, and that the uniqueness of levels of frozenness claim is unfounded in at least one major respect.[2]

The ordinary rules of pronominalization and deletion require that both anaphor and controller be referentiable and satisfy the particular conditions on identity of reference, identity of sense, etc. required by the rule involved. If idioms are subject to the same rules, then obviously the same requirements must be satisfied.

But first I must clarify further the appropriate coreference conditions for anaphora as they apply to idioms. For each idiom, there must be (as Fraser (1970: 26-32) shows) a full tree structure so that applicable transformations etc. can apply. This suggests that there are three tree structures relevant to idioms: one, call it SEM, which represents the semantic structure of the idiomatic reading of the idiom (i.e. what you really mean by it); another representing the semantic structure of the literal interpretation of the idiom; and finally one showing the point in the derivation where pronominalization (or whatever rule you are concerned with) applies. The diagram below shows the interrelationships:

(2)



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[2] While the counterexamples to the claim of idiom unanalyzability which are discussed here involve anaphora, there are also examples of clefting:

(17) a. It was my heart that she broke — not my will.
b. It's only face that I've lost.
c. It's the books that I'll hit tonight — not the bottle.
d. If it's a beef that you have with me, then say so.
e. It was a cold hard stare that she gave him.
f. It's his heart that John wears on his sleeve.

Furthermore, it is perhaps not unreasonable to regard sentences derived via conversational postulates (cf. Gordon and Lakoff, 1971) as special cases of idiomaticity, minimally constrained in substance and maximally analyzable. If one chooses to consider, for example

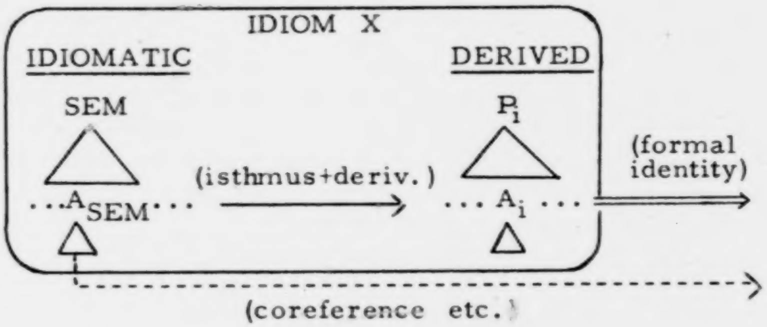
(18) It's a little cold in here, isn't it?

as an idiom when it has the force of 'please close the window', then such sentences are another, virtually unlimited source of pronominalization across idiom boundaries, since they are not particularly constrained. I do not pursue this line of inquiry further.

Within the tree SEM, there may exist some sub-tree A_{SEM} which corresponds to A_i (the derived candidate for controller or anaphor) in the following way: the mapping (i.e. idiom insertion) which maps SEM onto P_O (the semantic structure of the literal reading) also maps A_{SEM} onto A_O[3] (the sub-tree of P_O which "becomes" A_i in the course of the derivation). That is, A_i is interpreted as "really meaning" A_{SEM}. For example, in the idiom blow one's cool, some part of the (idiomatic) meaning like 'composure' is mapped onto cool. Whenever such an A_{SEM} exists for some A_i, we will call A_i REFERENTIABLE. We can then define coreference and other identity conditions for anaphora involving A_i via A_{SEM}. The mapping $A_{SEM} \rightarrow A_O$ we call the ANAPHORIC ISTHMUS, since unless it is defined, the idiom is an anaphoric island.

Thus no one should be surprised at the unacceptability of (1) above, since there is no referent for nose in pay through the nose nor in put (someone's) nose out of joint. Schematically, we have the relations below (where "--->" is the appropriate COREFERENCE relation; "⟶" is the ANAPHORIC ISTHMUS; and "⟹" is any requisite FORMAL IDENTITY):

(3)



In the case of anaphora between two idioms, the non-existence of either isthmus blocks anaphora.[4]

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[3] Technically, of course, the mapping $A_{SEM} \rightarrow A_O$ is the restriction of the mapping SEM $\rightarrow P_O$ to the sub-tree A_{SEM}.

[4] It might be proposed that the identity relations be defined between the A_O constituents of the idioms — i.e. the constituents corresponding to A_i in the semantic structures of the literal readings. This is incorrect, as shown by (1) above or by

(19) a. *The way John sticks his nose in Frank's business all the time, you'd suspect he'd cut it off to spite his face.
b. *Though Dick often puts his head in the lion's mouth, he sometimes buries it in the sand over trivial matters.

In these sentences there is the requisite identity on the literal reading (e.g. John's head = John's head), but pronominalization is blocked because the corresponding nodes in the idiomatic readings (for the case where they exist) lack such identity.

Pronominalization

The range of pronominalization across idiom boundaries is convincingly large. In cases of clearly anaphoric pronominalization, it is probably always true that the controller is within one idiom and the anaphor is also within an idiom (the same idiom, another token of it, or a different idiom). This is because the definition of IDIOM -- in terms of the surface/meaning mapping -- requires that a constituent identical to part of an idiom on the surface and also having the same meaning must be idiomatic, and these are necessary conditions for anaphora. What sometimes appear to be counterexamples are in fact discontinuities in the idiom rather than part of it:

(4) I've got John's number and he knows it.

To construct examples of pronominalization among idioms, all one needs are referentiable constituents in an idiom and another related idiom or repetition of the same idiom which can be put in the necessary syntactic relation for a rule of pronominalization. For example, in pass the buck, the buck means something like 'responsibility'. Hence:

(5) After the buck was passed to John, there was no one left to pass it to.

The first examples of pronominalization I consider involve identity of sense (i.e. of propositional function). Appendix A lists some idioms containing a possessed NP. Since the possessor in most such idioms is a discontinuity which is itself referentiable, the possessed NP (within the idiom) is often, perhaps always, also referentiable. Thus these idioms are a fruitful source for identity of sense pronominalization:

(6) a. Your days are numbered and so are mine.
b. It's a feather in his cap and in yours as well.[5]
c. He pulled my leg and I pulled his.

The list can be expanded without much imagination by anyone who wants to play with the idioms of Appendix A or others like them.

Some results are not acceptable, but for reasons independent of idiomaticity. For example,

(7) a. ?? John's bark is worse than his bite, and mine is worse than mine.

is awful for the same reason that

(7) b. ?? John's SPE is next to his dictionary, and mine is next to mine.

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[5] If one accepts an analysis of conjoined structures in which all such structures are derived via conjunction reduction (e.g. Tai, 1969), then all idioms with such structures must trivially involve the anaphoric process of identity deletion (or a more complicated process including the effect of identity deletion). The stronger claim that plurals arise in the same way (cf. Tai, 1969: 111 ff.) would include an even wider range of idiom phenomena.

is awful: a possessive pronoun (or adjective) which has a possibly coreferential surface clause-mate lowers acceptability — somehow it looks like sentence where reflexivization forgot to apply (e.g. *You are worse than you*). The sloppy identity in the sources of (7a.) and (7b.) allows the much better

(7) c. *John's bark* is worse than his bite, and *so* is *mine*.
d. *John's SPE* is *next to his dictionary*, and *so* is *mine*.

Such idioms (those containing possessed NP's) hardly exhaust identity of sense pronominalization. Appendix B (Idioms Containing Indefinite NP's) suggests examples like

(8) a. He tried to put *a spoke* in my wheel, but I managed to get *one* in his first.
b. He came in singing a merry *tune*, but he'll leave singing a different *one*.
c. He has a bone to pick with me, and I have *one* (to pick) with him.
d. They gave me a wide berth, but I gave them a wider one.[6]

Pronominalization among idioms can also involve indentity of reference:

(9) a. If you're gonna hit *the books* tonight, you'd better hit *'em* soon.
b. When he puts on *airs*, he puts *'em* on pretty good.
c. Mary often gets *the blues* when things like this happen, and she's sure got *'em* now.
d. Elvis played to *a full house* in Houston, and of course he brought *it* down.
e. Even if he straddled *the fence* for a while, at least he managed to come down on the right side of *it*.

That coreference is really involved is shown, for example, by the fact that in (9e.), the "fence" that is straddled must be the same "fence" that he comes down on the right side of — i.e., the same issue must be involved. Similarly, if we interpret (9b.) as ambiguous between readings where *put on airs* means 'be haughty' and where it means 'play songs over the radio (or whatever)', again there must be coreference. There are other pronominalization phenomena which are found less often, but I will not discuss them in detail.[7]

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[6] Here, as throughout, one must be careful that the requisite identity really is present. Thus

(20) ? ? He decided to turn over a new *leaf* and take *one* from John's book.

is bad because the two *leaf*'s are not the same kind of thing.

[7] In particular, there is abundant *so*-pronominalization, as exemplified by (6a.) and (7c.) in the text. *Do-so*-pronominalization suffers in its interaction with idioms from a stylistic conflict between the informal style of idioms and the more learned flavor imparted by *do so*:

(21) My father warned me not to let the grass *grow under my feet*, and I've been careful not to let it (__ / ?do so).

(continued on next page)

Deletion

Deletion phenomena across idioms show a similar range. First, consider Gapping:

(10) a. John gave a wide berth to Mary's old friends, and they __ an even wider berth/one to him.
b. Mary broke my heart, and Crystal, __ my will. [Not the legal kind!]
c. I'd like just once to see Rich put his head in the lion's mouth, and you, __ your money where your mouth is.

These examples may be open to the criticism that the gapped verbs are not really part of the idioms in question.[8] However, the structural description of Gapping requires a degree of parallelism that is usually found only in such marginally unit idioms. There are, however, a few slightly less dubious examples, such as

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(footnote 7 continued)

There are also idioms with optional internal pronominalization:

(22) step on it / step on the gas
see to it / see to NP
have it coming / have coming NP
it's just one of those things / NP's just one of those things
break it up / break up NP
ask for it / ask for NP
see how she blows / see how the wind blows

Though not central to the consequences of idiom pronominalization discussed in this paper, idioms with apparently obligatory pronominalization exist:

(23) as it were
at that
have had it
get it in the neck
hit it off
on the face of it
it's six of one and half-a-dozen of the other

[8] A similar possibility led me to the question of the relevance of the data discussed in Dong, 1971. In particular, as alleged evidence of outbound pronominalization from an idiom, he cites (p. 201):

(24) I have to take a shit$_i$ now, but after I've taken it$_i$, let's discuss this matter some more.

It is not clear that take a shit is properly an idiom in a narrow sense.

A possible alternative analysis is that it is the regular (within English) output of a rule segmentalizing a feature one might call [EVENTUAL], which indicates that the action in question is viewed as a countable event rather than a mass-like process. Besides a shit, one can take a look, vacation, walk, chance, drink, and many more. Though I will not pursue this in any detail, one might propose some rule like:

(25)
$$\begin{bmatrix} \text{V} \\ \text{+eventual} \\ \text{-stative} \\ \vdots \end{bmatrix}_{1} \Longrightarrow \text{TAKE} \begin{bmatrix} \text{N} \\ \text{+count} \\ \vdots \end{bmatrix}_{1} .$$

(11) John can't *tell* night from day, nor his brother, __ his ass from a hole in the ground.

The gapped *tell* is within the idioms, since the meaning is not that someone is incapable of telling or knowing something but rather that they are stupid.

Consider also:

(12) Dick and John are *up the creek* -- John, __ without a paddle.

Here the idiom is repeated in a variant form which is more emphatic, with the meaning that John is somehow in more of a fix than Dick is. Again, *up the creek without a paddle* seems to be a unit idiom.

Conjunction Reduction offers similar problems in finding unequivocal data. Parallel to (10a.-c.) are

(13) a. He *kept* his nose to the grindstone and __ his shoulder to the wheel.
b. He *killed* the goose that laid the golden egg and __ my chances as well.
c. He called attention __ and paid homage *to Oreo's tireless efforts.*

Note, of course, that identity constraints still apply to these idioms:

(14) a. **We took a lot of* time and __ guff.
b. *Eat shit and ___ your words!

Other types of deletion take place but will not be discussed:

(15) a. I'll put my head in *the lion's mouth* if you'll stick yours in __ too.
b. When we heard he was hopping on *the bandwagon*, we wanted on __ too.
c. *More* (power) to you!
d. Have (it) out with him.
e. And if I don't want to __ ?! [in reply to "You've made your bed -- now lie in it."]

Fraser's Frozenness Hierarchy

As noted at the beginning of this paper, Fraser claims that the fact that an idiom undergoes transformations specified for one of his levels (cf. fn. 1) implies susceptibility to all transformations specified for more frozen levels and conversely.

Though it is unclear to me where Fraser would want to put various anaphoric processes on his scale, it is clear that pronominalization is a process of at least level L-1 "Adjunction". Yet at least one idiom assigned to level L-0 "Completely Frozen ... no operations whatsoever may apply" exhibits pronominalization:

(16) John blew *his cool* and I blew *mine*.

Thus the uniqueness of levels hypothesis is falsified. In fact, the falsification is probably stronger than I have claimed, since pronominalization should probably be characterized at a higher level than "Adjunction" -- perhaps L-2 "Insertion" or L-5 "Reconstitution".

Regardless of the precise place of pronominalization in the Frozenness Hierarchy and regardless of the precise mechanism with which one captures the correspondence which is the anaphoric isthmus, the facts brought out in this paper show that idioms -- by definition highly idiosyncratic constructions -- are governed by the same conditions on anaphora as other, less idiosyncratic constructions.[9]

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[9] Since the original version of this paper was written, an interesting and, I think, relevant paper by Newmeyer (1972) has come to my attention. His principal claim is that idioms are not inserted as a unit, but rather the lexical items which compose the (surface) idiom are inserted (post-cyclically) as individual items, just as they would be if the literal reading were intended. Idiomaticity is captured by means of an "idiom inventory" consisting of meaning *pairs* (M_1, M_2) called "idiom sources"; M_1 is the real meaning of the idiom and M_2 is the meaning of the literal reading, so they correspond to my SEM and P_O, respectively. The derivation proceeds from M_2 but rules applying to it must meet syntactico-semantic constraints for both M_1 and M_2. Lexical entries are indexed for the idioms in which they occur by listing the appropriate idiom sources. Newmeyer produces a considerable body of syntactic and semantic evidence for at least the general lines of his analysis, which I will not examine in detail.

Though he focuses on movement rules, and specifically does not deal with anaphora, the spirit, if not details, of our investigations are, I believe, similar. His transderivational constraint based on idiom sources captures the intuition that the user of an idiom is simultaneously aware of its literal and idiomatic *meanings*. At the same time, the constraint seems to greatly reduce the amount of *ad hoc* irregularity involved in idiomaticity. My positing of the anaphoric isthmus has the same intentions. In fact, the isthmus is consistent with Newmeyer's analysis if the former is interpreted as the existence of an "idiom sub-source": an idiom source such that each tree of the pair is a sub-tree of the corresponding tree of another (single) idiom source. Given this identification, the conditions on anaphora are automatic consequences of the general transderivational constraint on idiom derivations. Newmeyer's analysis suffers most, as does my own, from a lack of details (e.g. in specifying the detailed forms that M_1 and M_2 must take). Nevertheless, I think the general insights gained should be an effective encouragement to the pursuit of those elusive further details.

APPENDIX A: IDIOMS CONTAINING POSSESSIVES

put all one's eggs in one basket
the apple of one's eye
have the ball at one's feet
s. o. 's bark is worse than his bite
have a bee in one's bonnet
bite s. o's head off
by the skin of one's teeth
throw (s.t.) in s. o. 's teeth
cast one's bread upon the waters
like to see the colour of s. o. 's money
nail one's colours to the mast
count one's chickens before they hatch
get s. o. 's dander up
for one's pains
twiddle one's thumbs
up s. o. 's alley
prick up one's ears
slip through s. o. 's fingers
out of one's depth
feather one's nest
feel in one's bones
slip through one's fingers
have a card up one's sleeves
call s. o. 's bluff
wash one's hands (of s.t.)
on one's toes
rest on one's laurels
pull one's weight
a run for one's money
steal s. o. 's thunder
sow one's wild oats
put that in one's pipe and smoke it
go out of one's way
win one's spurs
thank one's stars
that's s. o. 's funeral
get s. o. 's goat
make one's hair stand on end

APPENDIX B: IDIOMS CONTAINING INDEFINITE NP'S

an axe to grind
a bed of roses
turn over a new leaf
by a long shot
clutch at a straw
a chip off the old block
come a cropper
a tempest in a teapot
once in a blue moon
turn a deaf ear
for a song
have a screw loose
at a loose end
call it a day
for a rainy day
dead as a doornail
keep a straight face
within an ace
a far cry from
on a shoestring
a flash in the pan
a fish out of water
a square deal
take down a peg
wear a long face
by a hair
have a bee in one's bonnet
mad as a hatter
a little bird told me
a month of Sundays
steal a march on
knock into a cocked hat
wolf in sheep's clothing
a blue funk
a chip on one's shoulder
not a leg to stand on
ride for a fall
ring a bell
a near miss

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THE FEATURE [GRAVE] IN PHONOLOGICAL THEORY

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The acoustic feature [grave], included in the set of distinctive features proposed by Jakobson, Fant and Halle (1952), is defined as in (1) by Jakobson and Halle (1956):

(1) Grave/acute: acoustically - concentration of energy in the lower (vs. upper) frequencies of the spectrum

This feature of acoustic low tonality can be restated, though less satisfactorily, in articulatory terms, as seen in (2):

(2)	Peripheral ([+grave])	Medial ([-grave])
	Labial consonants	Dental/alveolar consonants
	Velar consonants	Palatal consonants
	Back vowels	Front vowels

There are two claims inherent in this feature distinction. First, labial and velar consonants (as opposed to dental and palatal consonants) are claimed to constitute a natural class; and second, labial and velar consonants pattern with back vowels, while dental and palatal consonants pattern with front vowels. In this paper I should like to provide evidence that both of these claims are correct, and that the acoustic feature [grave], which was discarded by Chomsky and Halle (1968), should be reincorporated into the set of universal phonetic features. I shall then turn to some of the implications of this proposal.

1. Weak Support for the Feature [Grave]

Surprisingly few arguments have been brought forth in support of the feature [grave]. Ladefoged (1971) and Vennemann and Ladefoged (1971) see evidence for such a patterning of peripheral consonants in the fact that [x] or [h] often become [f] historically, and vice-versa, as seen in (3):

(3) x > f cf. German lachen [laxən],
English laugh [læf]
f > h cf. Hausa dialects [fuɗu] and [huɗu] 'four'

Thus, one should compare the German word lachen [laxən] with the cognate English word laugh [læf]. This historical process of a fricative at one end of the oral cavity suggests a common feature shared by peripheral consonants. The force of this argument is somewhat weakened, however, by the fact that stops -- and even voiced fricatives -- apparently do not undergo this interchange as readily as voiceless fricatives. Also, it should be noted that fricatives can historically change into one another in a much more general way than stops, e.g. Proto-Niger-Kaduna *s has become [f] in Nupe -- cf. Gwari [ōsī́], Nupe [ēfī́] 'metal.' Finally, any voiceless fricative can become [h], e.g. Proto-Southwestern Mande *s becomes [h] in Mende — cf. Kpelle [sua], Mende [hùǎ] 'animal' (William E. Welmers, personal communication).

Notice that such a feature, if available, would simplify many phonological descriptions. In many languages, for instance, /l/ occurs only after labial and velar consonants (but not after alveolar and palatal consonants).[1]

If the feature [grave] is available, then the appropriate phonological constraint can be written as in (4a.) rather than as in (4b.):

(4) a.

$$\begin{array}{ll} \text{If} : & \#\begin{bmatrix}-\text{cont}\\ \text{C}\end{bmatrix}\ \ell \\ & \Downarrow \\ \text{Then} : & [+\text{grave}] \end{array}$$

b.

$$\begin{array}{ll} \text{If} : & \begin{bmatrix}-\text{cont}\\ \text{C}\end{bmatrix}\ \ell \\ & \Downarrow \\ \text{Then} : & \left\{\begin{matrix}\begin{bmatrix}+\text{ant}\\ -\text{cor}\end{bmatrix}\\ [+\text{back}]\end{matrix}\right\} \end{array}$$

One could, of course, simplify (4b.) by substituting the feature Labial for the feature specifications [+ant, -cor].[2] In fact, Vennemann and Ladefoged (1971) propose that the specification [+grave] actually stands for a disjunction involving the features Labial and Back, as in (5):

(5) $[+\text{grave}] \longleftrightarrow [+\text{labial}]$ or $[+\text{back}]$

The question in (4) above is whether this constraint has anything to do with graveness, an acoustic property, or whether it is just an accident that the absence of /l/ after alveolars and palatals creates the possibility of capturing this phonological constraint by means of the feature [grave]. In other words, there may be cases where graveness can be used to describe a phonological state, but where another phonetic motivation is responsible for this state.

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[1] Thus, Westermann (1930:19) says about Ewe: "r follows alveolar and palatal sounds, i.e. t, d, ts, dz, s, z, ny, and y, whereas l follows velar and labial consonants . . . ." We needn't be concerned here with whether this should be stated as a phonological constraint on l and r, or as a phonological rule which, say, converts /l/ to [r] in the appropriate environments.

[2] Strictly speaking, the specification [-cor] cannot be used to uniquely isolate labial and velar consonants, since Chomsky and Halle specify "true" palatals, as opposed to alveo-palatals, as [-cor]. However, Jim Hoard and Clarence Sloat (personal communication) have suggested to me that true palatals may best be viewed also as [+cor]. In any case, neither analysis allows us to capture the relationship between consonants and vowels which I shall discuss below.

Such a potential situation is represented in the hypothetical rule in (6):

$$(6)\quad \text{i} \longrightarrow \text{u} \;/\; \_\_\_ \begin{Bmatrix} \text{p} \\ \text{k} \end{Bmatrix}$$

In (6) the high front unrounded vowel [i] is backed and rounded to [u] before labials and velars. While this rule could be seen as involving graveness, as in (7),

$$(7)\quad \underset{\text{V}}{\begin{bmatrix} \text{+high} \\ \text{-grave} \end{bmatrix}} \longrightarrow \begin{bmatrix} \text{+grave} \\ \text{+round} \end{bmatrix} \quad / \;\_\_\_\; \underset{\text{C}}{[\text{+grave}]}$$

it is possible to view such a rule as being motivated by two articulatory assimilations, as in (8):

$$\begin{array}{llll} (8) & \text{a.} & \text{i} \longrightarrow \text{u} \;/\; \_\_\_\, \text{p} & \text{(labiality agreement)} \\ & \text{b.} & \text{i} \longrightarrow \text{u} \;/\; \_\_\_\, \text{k} & \text{(backness agreement)} \end{array}$$

In (8a.) [i] becomes [u] because of an assimilation to the labiality of [p]; in (8b.), [i] becomes [u] because of an assimilation to the backness of [k]. In both cases two features, [labial] and [back], are changed — when the vowel [i] becomes labial, it automatically becomes back; when the vowel [i] becomes back, it automatically becomes labial (cf. the linking conventions proposed by Chomsky and Halle (1968)). This then seems to be a good case of a rule which is conditioned by a disjunction of features, as seen in (9):

$$(9)\quad \underset{\text{V}}{\begin{bmatrix} \text{+high} \\ \text{-back} \\ \text{-labial} \end{bmatrix}} \longrightarrow \begin{bmatrix} \text{+back} \\ \text{+labial} \end{bmatrix} \quad / \;\_\_\_\; \underset{\text{C}}{\begin{Bmatrix} [\text{+back}] \\ [\text{+labial}] \end{Bmatrix}}$$

(A rule similar to this one is discussed in Section 2 below.)

Now compare the hypothetical rule in (10) with the one given above in (6):

$$(10)\quad \text{i} \longrightarrow \text{ɯ} \;/\; \_\_\_ \begin{Bmatrix} \text{p} \\ \text{k} \end{Bmatrix}$$

In (10) the high front unrounded vowel [i] is backed to the high back unrounded vowel [ɯ] before labials and velars. This time there is no labiality agreement and a rule must be written with the feature [grave], as seen in (11):

$$(11)\quad \begin{bmatrix} \text{+high} \\ \text{-grave} \end{bmatrix} \longrightarrow [\text{+grave}] \;/\; \_\_\_\; \underset{\text{C}}{[\text{+grave}]}$$

While the backing before velars could be construed as backness agreement, this would leave unanswered the problem of backing before labials. Thus, it is examples such as (10) which reveal both the similarity between labials and velar consonants, and the similarity between these peripheral consonants and back vowels. A very simple example of the backing of a vowel before grave consonants is seen in (12):

(12)	PFeˀ	*ab	>	Feˀfeˀ	[ɑp], e.g. [vɑp]	'to whip'
	PFeˀ	*ad	>	Feˀfeˀ	[at], e.g. [fat]	'to eat'
	PFeˀ	*ag	>	Feˀfeˀ	[ɑk], e.g. [čɑk]	'to seek'

Historically, the Proto-Feˀfeˀ front low vowel [a] was backed before syllable-final [p] and [k], but not before final [t]. The result is the distribution seen in (11).

In the following two sections I shall present data from what I term "high vowel reduplication" in two West African languages. In the first case, Igbo, it will be shown that the features Labial and Back can handle the facts (as seen in (9) above), while in the second case, Feˀfeˀ-Bamileke, the feature [grave] is unambiguously required.

2. High Vowel Reduplication in Igbo.

There are eight underlying vowels in Central Igbo, which have traditionally been divided into two sets of four vowels each on the basis of vowel harmony, as in (13):

(13) i u ị ụ

e o a ọ

The four vowels on the left can be recognized as [+advanced tongue root] and the four on the right as [-advanced tongue root] (Mona Lindau [personal communication]). The four vowels in the first row are [+high], while the four vowels in the second row are [-high]. I shall be concerned only with the unrounded stem vowels /i/, /e/, /ị/ and /a/, which reduplicate as seen in (14):

(14)	sí	'to cook'	⟶	ò-sísí	'cooking'
	sé	'to float'	⟶	ò-sísé	'floating'
	sị́	'to say'	⟶	ọ̀-sị́sị́	'saying'
	sá	'to wash'	⟶	ọ̀-sị́sá	'washing'

In (14) it is observed that front unrounded stem vowels take the high front unrounded reduplicated vowel [i] or [ị], depending on vowel harmony. In many dialects, however, this situation is modified by the presence of a labial stem consonant (cf. Hyman, 1973), as seen in (15):

(15)	bí	'to live'	⟶	ò-bíbí	'living'
	bè	'to cut'	⟶	ò-bù̱bè	'cutting'
	pị́	'to carve'	⟶	ọ̀-pị́pị́	'carving'
	bà	'to enter'	⟶	ò-bụ̱̀bà	'entering'

While this modification of [i] to [u] occurs only if the stem vowel is [-high], i.e. either /e/ or /a/, this limited data unequivocally shows the need for the feature [labial] in distinctive feature theory. Since the labial consonant can be any of those found in (16),

(16)	/p/	/f/	/kp/	/k^w/
	/b/		/gb/	/g^w/
	/m/		/w/	/$ŋ^w$/

the feature specification [+labial] will have to include bilabials, labiodentals, labiovelars and labialized velars, as well as rounded vowels (cf. Vennemann and Ladefoged, 1971). (For a formalization of the rules involved, see Hyman 1973.)

In a more restricted group of Igbo speakers, [u] and [ụ] replace the expected reduplicated vowels [i] and [ị] not only when the intervening consonant is labial, but also when it is velar. This modification is once again restricted to verbs whose stem vowel is [-high]:

(17)	kè	'to divide'	→	ò-kù̱kè	'dividing'
	ká	'to surpass'	→	ọ̀-kụ́ká	'surpassing'

(The expected forms [òkìkè] and [ọ̀kị́ká] are heard with much greater frequency, though this doesn't affect the argument for those speakers who use the forms in (17). Notice that Schachter and Fromkin (1968) report a similar variation in their study of Akan reduplication.) Since some speakers replace [i] and [ị] with [u] and [ụ] when the stem consonant is either labial or velar, we must now ask whether this is evidence for the feature [grave]. However, just as in the case of (6) above, it is possible to view the facts of (15) as labiality agreement, and those of (17) as backness agreement. Thus, these data do not suffice in themselves as proof for the need of the feature [grave].

3. High Vowel Reduplication in Feʔfeʔ.

While the data from Igbo reduplication can be interpreted in two ways, only one interpretation is possible of the data I shall now present from reduplication in Feʔfeʔ. From the examples in (18),

(18)	zα	'to eat'	→	zɯzα
	to	'to punch'	→	tɯto
	sɔ̍h	'to wash'	→	sɯsɔ̍h

it is seen that the general reduplicated vowel is the high back unrounded vowel [ɯ]. (This vowel is pronounced more centrally, i.e. as [ɨ], in rapid speech, though as [ɯ] when articulated carefully. The reduplicated forms have a number of uses in Feʔfeʔ, though I shall not be concerned with these here.) As seen in (19),

(19)	sii	'to spoil'	→	sisii
	pi̍i	'to profit'	→	pipi̍i

this high back unrounded vowel is modified to [i] when the stem vowel is [i]. When the stem vowel is [e], however, the following data in (20) are observed:

(20)	pée	'to hate'	→	pɯpée
	tee	'to remove'	→	titee
	yee	'to see'	→	yiyee
	kée	'to refuse'	→	kɯkée

The derivations in (21) are proposed (cf. Hyman, 1972):

(21)	(a)		(b)		(c)
	sii	→	sɯsii	→	sisii
	pii	→	pɯpii	→	pipi̍i
	pée	→	pɯpée		
	tee	→	tɯtee	→	titee
	yee	→	yɯyee	→	yiyee
	kée	→	kɯkée		

The input verb forms in (a) all reduplicate with the vowel [ɯ] in (b). This vowel becomes fronted to [i] in stage (c) if either 1) the stem vowel is [i], or 2) the stem vowel is [e] and the stem consonant is either dental or palatal, i.e. [-grave]. (For a formalization of the rules involved, see Hyman (1972.) In (22) we see that this second condition also holds when the stem vowel is [ɛ] or the front vowel [a]:

(22)	pɛn	'to accept	→	pɯpɛn
	tɛn	'to stand up'	→	titɛn
	čɛn	'to moan'	→	čičɛn
	ɣɛ́n	'to go'	→	ɣɯɣɛ́n
	paʔ	'to commit suicide'	→	pɯpaʔ
	taʔ	'to bargain'	→	titaʔ
	čaʔ	'to trample'	→	čičaʔ
	kaʔ	'to grill'	→	kɯkaʔ

We can thus generalize and say that whenever the stem vowel of the verb is [e], [ɛ] or [a] and the consonant is either alveolar or palatal, the reduplicated vowel is [i] rather than [ɯ]. As seen in (23),

(23)	čat	'to shatter'	→	čičat
BUT:	čɑk	'to seek'	→	čɯčɑk (*čičɑk)

no other stem vowel allows the modification of [ɯ] to [i]. As seen in (24),

(24)
$$\underset{\substack{[+\text{RED}]\\ \\ (\text{ɯ})}}{\underset{V}{\begin{bmatrix}+\text{high}\\+\text{grave}\end{bmatrix}}} \longrightarrow \underset{(\text{i})}{[-\text{grave}]} \;/\; \underline{\quad} \;\underset{(\text{t, č})}{\underset{C}{[-\text{grave}]}} \;\; \underset{(\text{e, ɛ, a})}{\underset{V}{[-\text{grave}]}}$$

these examples involve a change of [+grave] to [-grave] in the vowel and the required stem consonants are also [-grave]. Rule (24) is then a clear case of acoustic assimilation, suggesting that in addition to a notion of "ease of articulation," phonological theory requires a notion of "ease of perception" to explain assimilations. The naturalness of this process is considerably more explicit in (24) than in (25), where I have used the articulatory features of Chomsky and Halle (1968):

$$(25)\quad \begin{array}{c}\begin{bmatrix}+\text{high}\\ +\text{back}\end{bmatrix}\\ \text{V}\\ [+\text{RED}]\end{array} \longrightarrow [-\text{back}]\ /\ \_\_\_\ \begin{array}{c}\left\{\begin{array}{c}[+\text{cor}]\\ \begin{bmatrix}+\text{high}\\ -\text{back}\end{bmatrix}\end{array}\right\}\\ \text{C}\end{array}\ \begin{array}{c}[-\text{back}]\\ \text{V}\end{array}$$

Not only do we require the disjunction on the stem consonant, since palatals such as the glide [y] are [-cor] but still [-grave], but also it is not clear what [+cor] has to do with the assimilation at all, since all (non-retroflex) vowels are [-cor].

4. Implications for Distinctive Feature Theory

With the reintroduction of the feature [grave] into the inventory of distinctive features, a number of questions immediately come up. For instance, the question might be raised whether the existence of the feature [grave] makes it possible — or even necessary — to discard one (or more) of Chomsky and Halle's features, e. g. [coronal] or [back]. While I have produced evidence for this Jakobsonian feature, I have no evidence that any other related feature should be rejected. In fact, it is conceivable to me that we may wish to have features which partially overlap, such as [grave] and [coronal], and that each one may be found to play a role in a particular language. Conversely, while Feʔfeʔ requires the feature [grave] in order to reveal the motivation for the assimilation of reduplicated vowels, it is not necessarily the case that all languages will require such a feature. Thus, I conceive of a considerably large inventory of phonetic features, some of which overlap, from which languages choose a subset which they use phonologically. Or, in other words, some phonetic features are "phonologized," becoming part of the phonology, while other phonetic features are not. The feature [grave] must therefore be one of these phonetic features available to languages.

Having admitted the feature [grave], the next problem is knowing when to use it. It was already pointed out that the data from Igbo reduplication (Section 2) could be described from the standpoint of graveness assimilation, or labiality and backness assimilation. While I tend to think that here too we have an acoustic assimilation, it is still not clear to me how one might clearly choose one solution over the other. A similar problem arises in describing the history of English diphthongs. As pointed out by Stampe (1972), the diphthong [aw] does not occur before labials and velars in Modern English. A synchronic description of Modern English must therefore choose between the two ways of stating this negative constraint in (26):

(26) a. $\sim \quad \text{aw} \; \underset{\text{C}}{\begin{Bmatrix}[+\text{labial}] \\ [+\text{back}]\end{Bmatrix}}$

b. $\sim \quad \text{aw} \; \underset{\text{C}}{[+\text{grave}]}$

Historically, as pointed out by Stampe, the glide of the diphthong [uw] (the source of modern [aw]) was absorbed into following labials and velars, leaving a lax [ŭ] which later underwent the lowering part of the Great English Vowel Shift. Thus, from **ūt* we get modern English [awt], but from **ūp* we get [əp] (not **awp*, but cf. German *auf*) and from **dūvə* we get [dəv] (not **dawv*, but cf. German *Taube*). One hint that this absorption of [w] into a following labial or velar was articulatorily motivated is that the corresponding diphthong [ay] does not occur before palatals in Modern English, but *does* occur before dentals (e.g. **dīč* becomes [dɪč] and not **dayč* but cf. German *Deich*). Since the glide [y] is absorbed only into palatals, but not dentals, this must be due to articulatory homorganicity, rather than to acoustic assimilation. This means, of course, that /w/ will have to be specified in features as being a labiovelar, since it must be homorganic with both labials and velars.

However, a related problem is still unresolved in synchronic Hausa. Parsons (1970) (as reported by Schuh (1971)) notices that in Hausa, long /ii/ and the diphthong /ai/ are not found before dental and palatal consonants, and long /uu/ and the diphthong /au/ are not found before labial and velar consonants.[3] The analysis of /ii/ and /uu/ as /iy/ and /uw/, respectively, as suggested to me by Russell Schuh (personal communication), reveals the similarity of this case to the English one just discussed. Since /ai/ and /au/ are analyzed as /ay/ and /aw/ we can hypothesize that another case of glide-absorption took placd historically. But this time the glide /y/ is absorbed before dentals as well. Since dentals and palatal vowels are not viewed as homorganic in any sense in the current set of articulatory features, this seems to enhance the possibility that we have a case of acoustic absorption in this particular case.

5. Conclusion

Now that the feature [grave] has been shown to be a valid phonetic feature, it is up to phonologists to take a close look at ambiguous cases such as the ones discussed in this paper. I should like to conclude this paper by turning to a brief look at the intrinsic nature of graveness in vowels. It is often pointed out that the higher a front vowel is, the more palatal it is, and that the higher a back rounded vowel is, the more rounded it is. These phonetic truths are responsible for the fact that palatalization and labialization of consonants first occur before high vowels and later spread to mid vowels (and perhpas to low vowels). I should like now to present evidence for a related phonetic universal concerning graveness, as stated in (27):

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[3] Parsons mentions two exceptions to these generalizations: 1) recent cases of *uu* and *au* due to Klingenheben's Law, by which syllable-final labials and velars become *u*, e.g. *záunà* 'to sit (down)' (dialectal *zámnà*); 2) forms which derive from ablaut alternations, e.g. *dúukà*, verbal noun derived from *dòokáa* 'to beat.' In the first case we can presume that absorption took place historically before these cases of *uu* and *au* were created. In the second case, it appears that absorption is not applicable because the underlying form has only /oo/. Perhaps the exact phonetic realization [uu] was also created after the historical absorption rule.

(27) a. The higher a front vowel, the more acute (i.e. [-grave]) it is;
b. The higher a back rounded vowel, the more grave (i.e. [+grave]) it is.

I shall address myself only to (27a.).

In some Feʔfeʔ villages, instead of finding the data seen above in (20) and (22), the data in (28) are found:

(28) a. pée → pipée (cf. pɯpee)
tee → titee
yee → yiyee
kée → kikée (cf. kɯkee)

b. pɛn → pɯpɛn (= (22))
tɛn → titɛn
čɛn → čičɛn
γɛ́n → γɯγɛ́n

paʔ → pɯpaʔ
taʔ → titaʔ
čaʔ → čičaʔ
kaʔ → kɯkaʔ

In these villages (discussed at greater length in Hyman, 1972) the reduplicated vowel [ɯ] assimilates to [i] regardless of the consonant when the stem vowel is [e], as seen in (28a.). As seen in (28b.), however, the same distribution depending on the graveness of the stem consonant is found when the stem vowel is [ɛ] or [a]. What this means is that a certain "degree" of non-graveness is required to assimilate [ɯ] to [i]. In all villages, the stem vowel [i] is sufficient. In some villages, as just seen, the stem vowel [e] is sufficient. In others, it is sufficient only if accompanied by a [-grave] consonant. What is significant is that while some Feʔfeʔ speakers assimilate [ɯ] to [i] when the stem vowel is [e], but still require a [-grave] consonant when it is [ɛ], there are no speakers who always assimilate [ɯ] to [i] when the stem vowel is [e]. In other words, [e] is more [-grave] than is [ɛ].

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DISSIMILATION AS A NATURAL PROCESS IN PHONOLOGY

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1. Background

At least since the late nineteenth century, there has existed in European linguistics a tradition of considering dissimilation as a general process in language (Grammont, 1895; Meillet, 1936; Trubetzkoy, 1939; Posner, 1961; Sommerfelt, 1962).[1] In the writings of American scholars, however, assimilation is regarded as a widespread, general and "natural" process, while dissimilation is seen as restricted or unnatural. Linguistics textbooks have either treated dissimilation as a minor source of language change, along with haplology and metathesis (Whitney, 1867; Hockett, 1958; Gleason, 1961:85; Bolinger, 1968: 94; Arlotto, 1972: 87), or they have ignored it (Hoenigswald, 1960; Langacker, 1968; King, 1969). When the process is discussed, it is illustrated briefly with the l/r alternation in Latin, (peregrinus > pelegrinus), or with Grassmann's Law, operating in Sanskrit and Greek (Bloomfield, 1933: 349, 390; Lehmann,1973: 85, 167). Similarly, American linguistics journals contain numerous papers on assimilation (Schachter, 1969; Vennemann, 1972 are typical in this respect), but few on dissimilation. King expresses the general view, stating, "It may well turn out that most phonological change at bottom results from assimilation." (1969: 190).

Schane has recently proposed that assimilation is a natural rule in phonology. He questions, however, the possibility of considering dissimilation, as represented by Grassmann's Law, as a natural rule. Noting that many examples of dissimilative changes are sporadic, or operate in particular morphemes, he still wonders about what to do with the apparently phonetically regular Grassmann's Law. He concludes by saying, "Actually, one can say very little about how dissimilation is to be handled until there are more solid examples of this phenomenon." (Schane, 1972: 216).

What I should like to do here is to consider examples of phonetic and morphophonemic dissimilation. After providing "solid examples of this phenomenon," I will propose that dissimilation is natural and thus should be handled in phonological theory.

First, I should like to define dissimilation and naturalness, and show the relationship of dissimilation to assimilation, which many linguists claim is a natural process. Then, I should like to point out how dissimilation has a different motivation from assimilation, giving representative examples to support my claim.

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[1] I am grateful to Larry Hyman for valuable suggestions on an earlier version of this paper.

2. Definition

Dissimilation occurs when in the same word two segments share the same phonetic features, and one of the segments, usually the second of the pair, causes the other to either change one or more of its phonetic features (e.g. voice, continuancy) or to be deleted. The above-mentioned writings by European linguists have also pointed out the effect of contiguous consonants upon each other (called differentiation by Meillet). These examples, however, can be explained from another viewpoint, namely "weakening" (Foley, 1972). That is, stops are weakened to continuants intervocalically (ME fadar > ENE father). In a series of two consonants, the first of the pair is in a weaker position, undergoing change (Latin dictus > Ital. detto, not *dekko). Thus, apparent cases of dissimilation are more clearly seen as weakening (Ancient Greek epta > efta). I will not focus, then, on such examples, but rather will be concerned here primarily with non-contiguous segments.

Naturalness is usually defined on the basis of frequency of occurrence in language. Diachronically, natural rules or processes are those which have been frequently observed cross-linguistically by historical linguists. Synchronically, similar types of P-rules or processes are argued to be natural. It has also been claimed (Schane, 1972:199) that linguists have an intuitive notion of naturalness. Thus, assimilation is natural both diachronically and synchronically, and its frequency has many times been explained in the articulatory phonetic sense of "ease of articulation," with a more explicit explanation coming from experimental phonetics as "due to the co-ordination of different tongue muscles" (Schane, 1973:119).

Traugott states that linguistic changes usually involve either simplification or elaboration, and that a balance of these two opposing tendencies is significant in that if only simplification occurred, distinctions would be reduced. These distinctions, of course, are necessary to language (Traugott, 1972:14). Also, it has been explicitly claimed by Chomsky and Halle (1968: 178) that "phonological processes of assimilation and dissimilation" can be characterized with the "alpha variables," the use of which commits one "to the view that assimilation and dissimilation are not merely a matter of fortuitous coincidence of almost identical rules, but are, rather, linguistic universals." The alpha variable is thus often used in this sense (Harms, 1968; Shapiro, 1972).

Given that naturalness is a function of frequency of occurrence, and that this frequency requires some explanation, there appear to be at least three possible positions one might consider with respect to dissimilation. Thus, either (1) dissimilation -- like assimilation -- is natural because of articulatory ease, or (2) it is unnatural because it is articulatorily difficult, or (3) it is natural for some other reason. I should like to argue for this third view.

3. Relationship to Assimilation

What specifically is the relationship of dissimilation to (admittedly natural) assimilation? Are they, as has been claimed, "opposite" (Arlotto, 1972: 87) or the "reverse" (Antilla, 1972: 74) of each other? I would claim that they are not isomorphic opposites.

Consider, for example, the following case. While dental and velar consonants frequently assimilate to the palatality of the high front unrounded vowel, as seen in (1),

(1) $\left\{\begin{matrix} \text{t} \\ \text{k} \end{matrix}\right\} \rightarrow \text{č}/\text{i}$

Papago:	tia → čia	'hail'
Old English:	ik → ič	'I'
Slavic:	kito → čito	'what'

the opposite of this assimilation, in (2),

(2) $\text{č} \rightarrow \left\{\begin{matrix} \text{t} \\ \text{k} \end{matrix}\right\} / \text{i}$

is not found as a typical rule of dissimilation. That is, one cannot simply reverse the direction of the arrow in a natural assimilation rule and expect to obtain a natural dissimilation rule. Also, while (3) is frequent, (4) rarely, if ever, occurs.

(3) $V \rightarrow \tilde{V} / \_\_\_ \text{ nas}$

(4) $\tilde{V} \rightarrow V / \_\_\_ \text{ nas}$

Although the effects of dissimilation and assimilation are in a sense opposite, their domains of application are not comparable. Since these two processes are, therefore, not opposites, I would propose that they are motivated by different principles. It has frequently been claimed (Schachter, 1969; Vennemann, 1971) that assimilation, like other phonological processes, is motivated phonetically. Recently, "conceptual" factors have come to assume importance in phonological descriptions (Vennemann, 1971; Kiparsky, 1972). Generally, a contrast is drawn between these two motivations, the former allowing articulatory ease (irrespective of grammatical information), the latter preserving grammatical distinctions.

4. Motivation for Dissimilation

While I will not argue that morphemes need to be kept entirely distinct to prevent mergers (since mergers are common enough in language), I do claim that there is perceptual "value" in the "prominence" of segments within morphemes. (Following Chomsky and Halle (1968), I shall assume that both phonetic rules and rules requiring morphological information belong properly in the phonological component.)

I agree with Vennemann (1971: 22-24) that in discussions of linguistic change one must consider "motivations for change along with the functions of change in grammars as symbolization devices, with two ends, a conceptual and a phonetic one." Thus, there are two different simplificative tendencies in language, one leading to simplifications in the phonological structure (e.g. assimilation), the other leading to optimization of linguistic symbolization.

These tendencies are in conflict generally, as the former reduces distinctions in morphemes and the latter introduces them.

5. Examples

I should now like to turn to various examples of dissimilation, noting distinct differences between them. It appears from the literature that there are few cases of phonetic (that is, non-morphological) diachronic dissimilation. One is the familiar Grassmann's Law (Langendoen, 1966; Anderson, 1970), as shown in (5).

(5) $$\begin{bmatrix}-\text{son}\\-\text{cont}\end{bmatrix} \rightarrow [-\text{asp}]\ /\ \_\_\_\ \text{V} \begin{bmatrix}-\text{son}\\-\text{cont}\\+\text{asp}\end{bmatrix}$$

I-E		Skt	
*bhendh-	>	bandh-	'bind'
*phepheuga	>	pepheuga	'I have fled'
		Grk	
*thrikhos	>	trikhos	'hair'

There are also examples of phonetic dissimilation outside of Indo-European. According to Dyen (article reprint), a Proto-Austronesian sequence *s-s is dissimilated to t-s, as in (6).

(6) $$\underset{(s)}{\begin{bmatrix}-\text{son}\\+\text{ant}\\+\text{cor}\end{bmatrix}} \rightarrow \underset{(t)}{[-\text{cont}]}\ /\ \#\_\_\_\text{V} \underset{(s)}{\begin{bmatrix}-\text{son}\\+\text{ant}\\+\text{cor}\\+\text{cont}\end{bmatrix}}$$

PAN		Ngaju-Dayak	
*sisik	>	tisik	'fish scale'
*susu	>	tuso	'breast'

Iban (Sea-Dayak) also exhibits this dissimilative change.

Iban			PAN
tisil	'cut'	<	*s-s
tusun	'crowd'		

One other example of phonetic dissimilation, called Ganda Law, emphatically stated by Meinhof (1932) as a general phonetic rule, has been shown by Meeussen (1962) to be restricted in application to voiced labials, velars and liquids in nominals belonging to a fixed class.

There is, however, the clear case of phonetic dissimilation called Dahl's Law, also explicitly stated by Meinhof and confirmed by Guthrie (1948:45), Tucker and Bryan (1957:48, 55) and Kahler-Meyer (1971). Similar to Grassman's Law, Dahl's Law states that "when two successive syllables each begin with an aspirate the first of these loses its aspiration and becomes voiced" (Meinhof,1932:181). The law operates in some Benue-Congo languages, such as Nyamwezi, Shambala, and to a lesser degree in Kikuyu, as in (7).

(7) Dahl's Law

$$\begin{bmatrix}-\text{son}\\-\text{cont}\end{bmatrix} \longrightarrow [+\text{voice}] \;/\; \_\_\_\; \text{V} \begin{bmatrix}-\text{son}\\-\text{cont}\\-\text{voice}\end{bmatrix}$$

Proto-Bantu		Nyamwezi	
*-kati	>	-gathi	'in the middle'
*-tatu	>	-dathu	'three'
*-pita	>	-bitha	'pass'

(Note that *th* is an aspirate from original *t*, as are *ph* and *kh* < *p*, *k*, showing that aspiration was the crucial dissimilatory feature, i.e. **kati* > **khathi* > *kathi* > *gathi*, thus [-asp] ⊃ [+voice].)

According to Bennett (1957), Dahl's Law also operates in Embu, Mwimbi and Tharaka, though restricted to velar obstruents (for example, Southern Kikuyu *gukua* 'to deal' < **kukua*).

These are the only clear examples of truly phonetic diachronic dissimilation that I have found in a vast literature on this topic. Thus, in terms of naturalness, as compared with the great frequency of historical assimilation rules on the phonetic level, these examples must be tentatively judged as infrequent.

From here on, I will be concerned with dissimilation diachronically and synchronically as a natural process in the sense that it is a perceptual universal, that is, a natural rule whose function is to enhance the prominence of concatenated morphemes. As will be seen in the following examples, this entails distinguishing affixes from stems — or in other words, making stem boundaries more prominent.

There are, of course, numerous examples of *l/r* dissimilation in Latin and the Romance languages. As Posner reports, every conceivable modification of this phenomenon has been observed (1961:105). For example, Latin *fragrare* 'fragrant': in Catalan the first *r* is deleted, while in French it appears as *l*; in Spanish the second *r* is deleted. In Latin *frigorosum*, the second *r* appears as *l* in French (*frileux* 'chilly'). In the Latin present participle *fragrans*, both *r*'s are preserved in the English borrowing, 'fragrant.' Though the usual direction of dissimilation is regressive (as is the usual direction of assimilation) (Kent, 1936), a number of progressive dissimilations have been cited. (The multiplicity of changes in the *l/r* alternation is what caused Grammont and others after him to attempt to posit numerous *ad hoc* principles to deal with observed forms.)

Perhaps the answer for the instability of *l* and *r* together in a word can best be explained by some non-dissimilative factor, such as "strength" or some (physical) phonetic factor. However, the clearest cases of *l/r* dissimilation are those in which stems are attached to suffixes, as can be seen in (8).

(8) $$\begin{bmatrix}+\text{cons}\\+\text{voc}\end{bmatrix} \rightarrow [-\text{lat}] \;/\; \begin{bmatrix}+\text{cons}\\+\text{voc}\\+\text{lat}\end{bmatrix} \# \underset{[+\text{suffix}]}{\underline{\qquad}}$$

Latin
libera-lis, morta-lis (stem r and suffix l)
familia-ris, popula-ris (stem l and suffix r)

This is still a productive process in English, as pointed out by Ross (reported by Kiparsky, 1972: 216), where dissimilation operates on the adjective-forming suffix -al → -ar when attached to verbs with an l in the stem ('cellular,' 'circular'), and a constraint on derivational morphology preventing the nominalizing suffix from being added to verbs containing l ('betrayal,' 'rehearsal,' 'burial,' but not *applial, *allowal). As can be seen in the Latin example, then, the prominence of the different liquid in the suffix, caused by perceptual needs, is the motivating factor for this dissimilation. That is, a form *populalis would lack this distinctiveness and thus the suffixal l would be a poor marker for the suffix. All other cases of l/r dissimilation are truly sporadic, for example, English 'turtle' < L. turtur, 'purple' < L. purpre, but 'brother': L. frater, 'crater': L. crātēr.

Dissimilation can be found in a number of American Indian languages. All cases I am familiar with, however, are restricted to concatenated morphemes. For example, in Narragansett and closely related Natick, members of the Algonquin family, the nasal in the locative suffix, reconstructed by Bloomfield as Proto-Algonquin *-enki, was deleted when there was a nasal in the stem (Hamp, 1970). Thus in (9):

(9) Proto-Algonquin — Narr
*aθankwa 'star' > *anankwa > anockqus
↓
∅ — Nat
anoqqs

(Proto-Algonquin *θ > Narr, Nat, Menomini, Fox n; confirmed by Haas, 1967)

Also, in Narragansett, $k \rightarrow t / kV \underset{[+\text{loc}]}{\underline{\qquad}} \#$

Examples from Cowen (1969) are:

Annaquatuket Conanicut Woonsoket

The first example in (9) demonstrates a perceptual strategy, that is, ∅ in the suffix "tells" speakers there is a nasal in the stem. Thus the stem is highlighted, as it also is by the final t in the second example.

Gudchinsky and Popovich (1970) have shown dissimilation to be operative in Maxakali, where the second of a sequence of two nasals becomes devoiced -- a case of voicing dissimilation. The examples in (10) are:

(10)	kõnnĩn 'macaw'	+	nõʔõm 'that'	kõnnĩnn̥õʔõm
	kõnnĩn	+	mĩnnĩ 'black'	kõnnĩnm̥ĩnnĩ
	kõmãn 'co-godmother'	+	ŋãñ 'angry'	kõmãnŋ̊ãñ

Notice here, too, that the devoicing of the initial nasal in the adjective intensifies, via contrast with the final nasal of the noun, and maximizes the adjective. Thus, in rule form as in (11).

(11) $$\begin{bmatrix}-\text{voc}\\+\text{nas}\end{bmatrix} \longrightarrow [-\text{voice}] \;/\; \begin{bmatrix}-\text{voc}\\+\text{nas}\\+\text{voice}\end{bmatrix}\ \#\underset{[+\text{adj}]}{\underline{\qquad}}$$

A further example of dissimilation is taken from Tucker and Bryan's (1964) extensive study of Nandi, a Southern Nilotic language. In Nandi (and closely related Kipsigi), each noun has a primary (short) form and a secondary (long) form. The secondary form ends in a suffix containing -t̲- (or -d̲-) in the singular and -k̲- (or -g̲-) in the plural. Secondary affixes are classified according to long (Class A) and short (Class B).

In certain Class A nouns, especially those with disyllabic stems or a Primary Number suffix, the secondary suffix preserves length only if the preceding vowel is short; if the preceding vowel is long, the secondary suffix is short. Thus, in (12):

(12)		Primary	Secondary	
	Nandi:	kɪ̀mɪ́t	kɪ̀mɪ̀t-ɛ̂:k	'fleas'
		tárɪ̂:t	tàrɪ̀:ty-ɛ̀t	'bird'

Thus the rule for Nandi (and Kipsigi) is (13):

(13) $$V \longrightarrow [-\alpha\text{long}] \;/\; \begin{bmatrix}V\\ \alpha\text{long}\end{bmatrix}\ C\underset{[+\text{suffix}]}{\underline{\qquad}}$$

There has been much discussion about "polarization" and dissimilation in African tone languages (Spears, 1968, 1971; Leben, 1971). For example, Leben argues for a tonal dissimilation rule in the Chadic language Hausa, shown in (14).

(14) $$\text{Low}\quad \underset{[+\text{long}]}{\text{Low}}\quad \# \longrightarrow \text{Low}\quad \underset{[+\text{long}]}{\text{High}}\quad \#$$

/káràntàa/ → [káràntáa] 'reading'

Assuming this is a valid diachronic and synchronic rule in Hausa, it appears to be, like Grassmann's Law, a phonetic rule.

There are, however, cases where what appears to be tonal dissimilation may have a different historical explanation. Hyman and Schuh (1972: 30-31) posit an explanation for a case in Feʔfeʔ-Bamileke. In Feʔfeʔ one finds a raised-low tone before a high tone (or a pause), but a rising tone before a low tone. There appears to be a rule, as in (15):

(15) raised-low → rising/___low tone

Yet consider the following derivations in (16):

(16)	a	b	c	
	*cὰ́k	cα̍́k	cα̍k	'pot'
	*cὰ́k lά́	cα̍́k lά́	cα̍k lά́	'that pot'
	*cὰ́k lè	cα̍́k lè	cα̌k lè	'this pot'

(́ = High, ̀ = Low, ' = Raised Low, ̌ = Rising)

In stage a, raised-low nouns are reconstructed with L-H tone. Stage b shows that the low tone is raised to a raised-low tone (before a high tone). In stage c, the high tone is deleted before a pause (as in the citation form 'pot') or before a high tone, as in 'that pot.' Before a low tone, the raised-low tone becomes a rising tone (as in 'this pot'). Rather than a case of dissimilation, Hyman and Schuh argue that the explanation is the loss of the high part of the rise. They suggest that the rule in Mandarin (McCawley, 1970) by which L-L becomes R-L may have a similar explanation.

Other cases I have examined (such as Meinhof's (1932) dissimilative "Kuanyama Law," Winter's (1970) dissimilation in reduplicative forms in Washo; Bucca and Lester's (1970) nasal dissimilation in Kitsai, Sommerfelt's (1962) example in Irish) conform to my claim; that is, while phonetic dissimilation is rare, morphophonemic dissimilation occurs frequently. Notice that even Grassmann's Law, when it operates synchronically (Anderson, 1970), can also be argued as perceptually motivated. For example, in Sanskrit reduplicated perfects, shown in (17):

(17)	Root		Present	Perfect
	phal	'burn'	phalari	paphela
	khād	'chew'	khādan	cakhāda
	dhauk	'approach'	dhaukati	dudhaka

The rule could be formulated as in (18).

$$(18)\quad \begin{bmatrix}\text{-son}\\ \text{-cont}\end{bmatrix} \rightarrow [\text{-asp}] \; / \; \underset{[\text{+prefix}]}{\underline{\qquad}} \; V \begin{bmatrix}\text{-son}\\ \text{-cont}\\ \text{+asp}\end{bmatrix}$$

As opposed to the few phonetic dissimilation rules (examples 7, 8 and 9), rules (12), (15), (17), (18) and (24) must include grammatical information.

As mentioned above, cases of change between contiguous segments within morphemes can be explained by weakening, or as a result of articulatory processes. What appears, then, as a dissimilatory state (Greek ftero < pteron) may not be due to a dissimilatory process. Or it may be argued that when contiguous segments become dissimilar, this is due to perceptual factors, to preserve distinctions within morphemes. Only when two contiguous segments are separated by a word boundary (as in example (10)) does dissimilation, as I have considered it here, occur.

6. Conclusion

What I hope to have demonstrated, then, is that while assimilation is phonetically motivated, dissimilation is motivated by perceptual (psychological) factors, and is a natural process which operates to preserve the distinctiveness of the stem-affix relationship. The tendency of speakers to highlight this stem-affix relationship should therefore be integrated into the metatheory, just as the tendency of speakers to phonetically assimilate contiguous segments has been accommodated. Thus, along with assimilation, dissimilation should be viewed as natural in phonological theory.

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CONDITIONS ON EXTRACTION RULES: THE CASE FROM ZAPOTEC

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Introduction

In his article "Conditions on Transformations" Chomsky (1971) has proposed a system of conditions which have the effect of limiting the power of extraction rules. An extraction rule is a transformation which has the power to move a constituent into a higher S. These conditions are formalized on the basis of data from English, but Chomsky seems to suggest that they might also be assigned to universal grammar. After briefly presenting those conditions which are relevant, we will examine two Zapotec extraction rules, Focus Movement and Emphasis Movement, and show that the effect of these rules is inconsistent with Chomsky's system of conditions.[1] That is, a Zapotec grammar which includes these conditions will be unable to meet observational adequacy. Finally, we will consider these two rules in terms of the general issue of whether they apply on successive cycles or in a single unbounded movement.

Conditions on Extraction Rules

Basic to Chomsky's system of conditions is the assumption of the existence of a universal element Comp and that the following set of base rules are included in the grammar of English:

(1) i. $S \rightarrow$ Comp S'
ii. $S' \rightarrow$ NP Aux VP
iii. Comp $\rightarrow$ P NP $\pm$ WH

Phrase markers generated by the set of base rules which include (1) will have the node Comp in S initial position for both root and embedded Ss. The following conditions are also required:

(2) a. The base rules can not insert a lexical item into Comp; it can only be filled by a Comp insertion transformation. (p. 6)

b. Once a Comp insertion transformation has applied to a Comp node, no subsequent Comp insertion transformation can insert a constituent under that node. (p. 14)

And Chomsky assumes that:

(3) S (but not S') is the domain of cyclic rules (p. 14).

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[1] The dialect of Zapotec under investigation is spoken in the center of the Valley of Oaxaca between the cities of Oaxaca and Mitla. Field research for this study was conducted in Oaxaca, Mexico, 1970-71 and supported by grants from the Institute of Latin American Studies, University of Texas; American Philosophical Society; National Science Foundation Grant GS 2468. I wish to thank my main informant, Abel Hernandez, for his patience and interest and Robert Berdan and Stanley E. Legum for their discussions of theoretical points.

There are also the following conditions:

(4) No rule can involve X, Y in

... X ... [α ... Z ... WYV ...] ...

where i. Z is the specified subject of WYV
or ii. Y is in Comp and X is not in Comp
or iii. Y is not in Comp and α is a tensed S

The general effect of (4) is to impose the limitation that an item can not be extracted from an S if it crosses over the specified subject of that S, nor can it be extracted from a tensed S if it is not in a Comp node. In addition, once an item is dominated by a Comp node it can only be moved into another Comp position. By a specified subject, Chomsky means a subject NP that is a lexical item or a non-anaphoric pronoun. By tensed S he means an S containing a finite verb.

The tensed S Condition (4.iii.) is illustrated by the application of the Passive in sentences (5) and (6):

(5) a. [$_S$ I believe [$_S$ the dog is hungry.]]
b. *The dog is believed is hungry (by me).

(6) a. [$_S$ I believed [$_S$ the dog to be hungry.]]
b. The dog is believed to be hungry (by me).

In (5b.) the Passive extraction of the NP 'the dog' from the tensed complement S is blocked by condition (4.iii.), but Passive extraction is not blocked in (6b.) since the form of the verb in the embedded S is not finite. Looking ahead for a moment, all Zapotec main verbs are inflected with an aspect marker, thus making them finite.

The final condition on extraction rules relevant to this presentation is (7):

(7) No rule can involve X, Y X superior to Y if Y is not subjacent to X. (p.18)

The subjacent relation is defined as that structure in which X is superior to Y and there is at most one cyclic category $C \neq Y$ such that C contains Y and C does not contain X. This condition restricts the application of extraction rules to adjacent or successive cycles. Given the further assumption that "both NP and S are nodes to which cyclic operations apply (p. 8)," Chomsky uses (7) to account for being able to derive (8b.), but not (8c.), from (8a.):[2]

(8) a. COMP he considered [$_{NP}$ the question [$_S$ COMP John saw who]]
b. COMP he considered [$_{NP}$ the question [$_S$ [$_{COMP}$ who] John saw]]
He considered the question who John saw.
c. [$_{COMP}$ who] he considered [$_{NP}$ the question [$_S$ Comp John saw]]
*Who did he consider the question John saw?

---

[2] Neither (8b.) nor (8c.) are acceptable sentences in my dialect.

On the root S cycle who cannot be extracted out of the embedded COMP node of (8b.) to derive (8c.) because there is more than one intervening cyclic node between it and the root COMP (i.e. S and NP).

Focus Movement

In Zapotec, there are a number of possible surface orders of main constituents. The unmarked surface order, however, is VSO. By unmarked order I mean that order of constituents found in declarative sentences which do not contain negation, or mark a specific constituent or constituents for focus, emphasis, or topicalization. The occurring and non-occurring Zapotec surface orders are given in (9):

(9) VSO SOV
SVO *VOS
OVS *OSV

Following Halliday (1967) and Chomsky (1968) the term focus is used to refer to the surface structure constituent that contains the new information in the sentence. Focused constituents in Zapotec only occur in preverbal position as illustrated by the following sentences:[3]

(10) a. tu gudo biza
who ate beans 'Who ate the beans?'

b. xwain gudo biza (SVO)
Juan ate beans 'Juan ate the beans.'

c. # gudo xwain biza (VSO)
ate Juan beans

(11) a. ši gudo xwain
what ate Juan 'What did Juan eat?'

b. biza gudo xwain (OVS)
beans ate Juan 'Juan ate the beans.'

c. # gudo xwain biza (VSO)
ate Juan beans

d. # xwain gudo biza (SVO)
Juan ate beans

The above (b.) sentences are acceptable answers to the corresponding (a) sentences. Sentences (10c.), (11c.), and (11d.) are not ungrammatical in themselves, but they are incorrect as answers to their respective questions. Sentence (11d.) in this situation would be the equivalent of answering 'What did John eat?' with 'It was John who ate the beans.' The surface order SOV occurs in answers to multiple questions:

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[3] The symbol # indicates that the sentence is inappropriate or deviant in the given context. It is acceptable in a different context.

(12) a. tu de go ši
Who plural will-eat what
'Who will eat what?'

b. de buink biza go ne de buinre7
plural person-those beans will-eat and plural person-those
bel go
meat will-eat
'These people will eat beans and those people will eat meat.

Rosenbaum (1973) presents a number of arguments that the deep order in Zapotec is VSO. I will assume this to be the case and that focused structures are derived by a movement rule that places focused constituent before the verb.

The Focus Movement rule is given in (13):

(13) Focus Movement (obligatory)

X	V	Y	NP	Z	
			[+Focus]		
1	2	3	4	5	⟹
1	4+2	3	0	5	

Conditions: 3 cannot contain the feature [+Focus]

I will assume that the distinction between focused and non-focused constituents is made in the deep structure. I have argued (Rosenbaum 1973) that this deep structure distinction is necessary in order to provide the correct semantic interpretation of the resulting string. The feature [+Focus] is introduced to the appropriate constituent by a transformation.[4] The presence of the feature and the condition on the rule are necessary to prevent the generation of the unacceptable OSV sequence when the rule applies iteratively as in sentence (12b.). Notice that the SOV surface order can only be derived by a Focus Movement rule which is allowed to apply iteratively; that is, to its own output. On the first application of the rule the subject will be brought before the verb. On the second application, the object is placed before the verb.

Turning now to the extraction properties of Focus Movement we find that focused constituents must be moved to the highest S as in the following (b) sentences:

(14) a. tu bakwaž nie lani
who prohibited-he incompleted-go party
'Who did he prohibit from going to the party?'

---

[4] The claim that the semantic distinction between focused and non-focused constituents is determined in the deep structure is not crucial for the arguments presented in this paper. These arguments would also hold under a surface structure interpretation approach. This same point also holds for the arguments made about the rule of Emphasis Movement in the following section.

b. abel bakwaž nie lani
Abel prohibited-he incompleted-go party
'He prohibited Abel from going to the party.'

c. # $[_S$ bakwaž $[_S$ abel nie lani]]

(15) a. tu rkaž če ldua7
who wants-he go Oaxaca 'Who does he want to go to Oaxaca?'

b. xwain rkaž če ldua7
Juan wants-he go Oaxaca 'He wants Juan to go to Oaxaca.'

c. # $[_S$ rkaž $[_S$ xwain če ldua7]]

Sentences (14c.) and (15c.) are not appropriate answers to (14a.) and (15a.) respectively. This means that under Chomsky's system of constraints, the Focus Movement transformation is a Comp insertion rule. Chomsky does not indicate how a Comp insertion rule is to be formulated. The following reformulation of Focus Movement seems like a reasonable example:[5]

(16) Focus Movement reformulated as a Comp insertion rule (obligatory)

$[_{COMP}$ NP	X]	V	Y	NP [+Focus]	Z	
1	2	3	4	5	6	⟹
5 +	2	3	4	∅	6	

Conditions: 4 cannot contain the feature [+Focus]

Reviewing the examples of Focus Movement which we have considered we find that the Focus Movement rule as reformulated in (16) will derive the (b.) sentences of (10), (11), (14), and (15) all of which have only one focused constituent. However, it is unable to derive sentence (12b.) with its SOV order because of the prohibition on inserting more than one element into COMP. This prohibition has the effect of not allowing the rule of Focus Movement to apply interatively. One ad hoc way of getting around this problem is to propose two distinct Focus Movement rules: one rule, identical to (16) which places focused constituents in COMP for possible subsequent extraction, and a second rule (similar to (13)) which places focused constituents after the COMP constituent and before the verb. The sequence in (17) illustrates this proposal by deriving the first conjunct of (12b.) from its underlying string:

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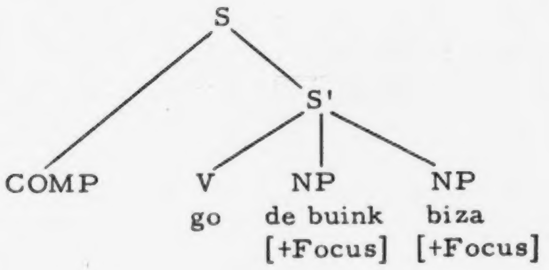
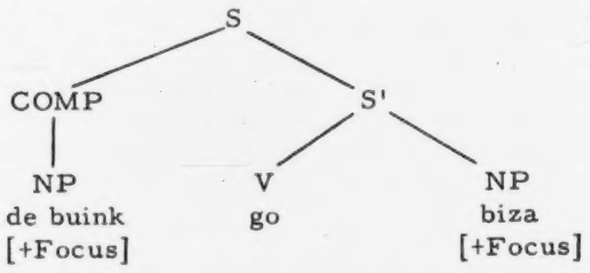
[5] Since Chomsky's conditions are intended as general conditions on extraction rules, not as conditions on specific rules, he might formulate (16) without the information in the S.D. that elements 1 and 2 are dominated by the COMP node. This information would be included in the grammar as part of the general conditions on extraction rules. My formulation in (16) is a notational equivalent and does not affect the following arguments in any way.

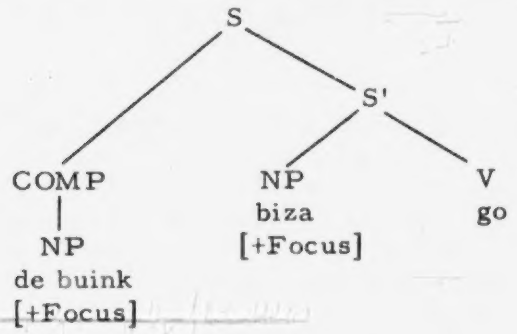
(17) a.



b. Focus Movement (16)



c. Focus Movement (13)



However, the above proposal is empirically inadequate because it is unable to account for sentences in which both subject and object are extracted into the highest S as in (18b.):

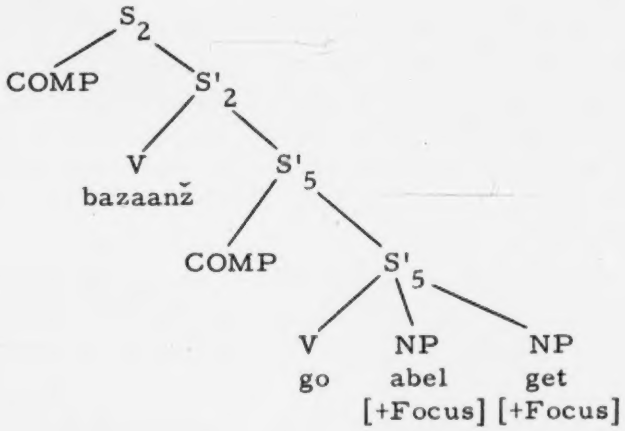
(18) a. tu de bazaanž gudo ši
who plural allowed-he ate what
'Who did he allow to eat what?'

b. [$_S$ abel get bazaanž [$_S$ gudo]] [xwain biza][$_S$ ne makU bel]
Abel tortillia allowed-he ate Juan beans and Markos meat
'He allowed Abel to eat tortillas, Juan beans, and Markos meat.'

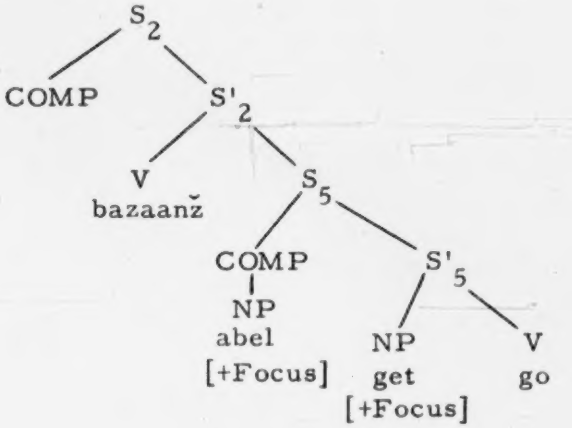
c. #[$_S$ abel bazaanž [$_S$ get gudo]] xwain biza ne makU bel
Abel allowed-he tortilla ate Juan beans and Markos meat

The underlying structure of the first conjunct in (18b.) can be represented as (19a.):

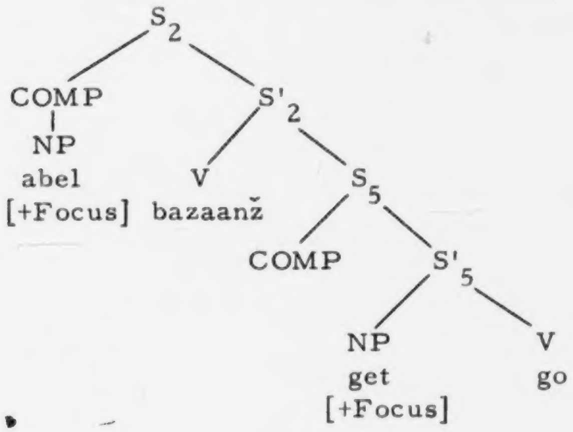
(19) a.



b. Application on the S_5 cycle of the rules of Focus Movement (16) and (12)



c. Focus Movement (16) on the S_2 cycle yields the structure for (18c.)



Under Chomsky's set of conditions we can not derive (18b.) since no additional applications of the Focus Movement rules are possible. The tensed S condition (4.iii.) prevents get from being extracted out of S_5. Condition (2b.) prohibits get from being placed in the vacated Comp node of S_5 for subsequent extraction. Even if get could be extracted from S_5, under conditions (2b.) and (4.ii.) it can not be moved into S_2 because the Comp node in S_2 contains the NP abel.

Therefore, Chomsky's system of conditions on extraction transformations is incompatible with the grammar of Zapotec. A Zapotec grammar containing these conditions would be unable to meet observational adequacy.

Emphasis Movement

Turning now to the phenomenon which I will call Emphasis we find that constituents must be placed before the verb when they are in contrast with other constituents in the sentence. Consider the following sentences:

(20) a. xwain we ldua7 ati abel (SVO)
Juan went Ozxaca not Abel
'Juan went to Oaxaca not Abel.'

b. *we xwain ldua7 ati abel
went Juan Oaxaca not Abel

(21) a. biza budo xwain ati bel (OVS)
beans ate Juan not meat
'Juan ate beans not meat.'

b. * gudo xwain biza ati bel
ate Juan beans not meat

The (b.) sentences of (20) and (21) are ungrammatical; preposing the contrasting constituent is obligatory. Not all instances of Emphasis, however, involve contrasting constituents. In the following dialogue the word xwain in Abel's sentence is not in contrast with any constituent in that sentence or the context of the conversation:

(22) Markos: šinii gudo xwain škestilia7
why ate Juan my-bread
'Why did Juan eat my bread?'

Abel: 070 xwain get gudo ati gestilE (SOV)
no. Juan tortilla ate not bread
'No. Juan ate a tortilla, not bread.'

The rule of Emphasis Movement is similar in many respects to the rule of Focus Movement:

(23) Emphasis Movement (obligatory)

X	V	Y	NP [+Emphasis]	Z	
1	2	3	4	5	$\Rightarrow$
1	4+2	3	$\emptyset$	5	

Conditions: 3 cannot contain the feature [+Emphasis]

As was the case with the rule of Focus Movement the feature [+Emphasis] is introduced by a transformational rule on the basis of a deep structure distinction. I have argued (Rosenbaum, 1973) that this distinction is required to provide for the correct semantic interpretation of the string. Likewise, the feature [+Emphasis] is also necessary in conjunction with the condition on the rule to maintain the correct order of emphasized constituents where the rule applies iteratively as in (22).

Emphasis Movement, like Focus Movement, is an extraction rule:

(24) xwain rkaž če ldua7 ati abel
Juan wants-he go Oaxaca not Abel
'He wants Juan to go to Oaxaca not Abel.'

This means that the system of constraints proposed by Chomsky is also incompatible with the rule of Emphasis Movement since this rule reformulated as a Comp insertion rule would be unable to generate SOV sentences like Abel's answer in (22).

There is a difference, however, between the Emphasis Movement rule and the Focus Movement rule. The extraction properties of Emphasis movement are optional. Sentence (25) is as acceptable as sentence (24).

(25) rkaž xwain če ldua7 ati abel
wants-he Juan go Oaxaca not Abel
'He wants Juan to go to Oaxaca not Abel.'

We can account for this difference by adding condition 2 to the rule given in (23):

(26) Emphasis Movement revised

X	V	Y	NP	Z	
			[+Emphasis]		
1	2	3	4	5	⇒
1	4+2	3	∅	5	

Conditions: 1) 3 cannot contain the feature [+Emphasis]
2) obligatory when 2 and 4 are immediately dominated by the same S, otherwise optional

This condition will require that all emphasized constituents are obligatorily moved in front of the verb in the S in which they originate, but will allow extraction to be optional. Condition 2 is a little unusual since most rules are either obligatory or optional, but not both.[6] The unusual feature of this rule seems plausible when we consider the rule's function. The basic phenomenon of emphasis is signaled by movement. This is obligatory just as is the extra heavy stress and higher pitch that signals emphasis in English. The optional extraction function of the Emphasis rule seems similar to the effect of the optional rule of Extraposition in English which creates a subtle difference in meaning or intent between the following (a.) and (b.) sentences:

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[6] The obvious alternative to condition 2 is two separate rules of Emphasis Movement. There is little support for such a proposal since it provides no additional advantages and contains a clear disadvantage. Assuming that obligatory Emphasis Movement is a cyclic rule, it will require a condition, similar in part to condition 2, that limits its application to instances where

(27) a. It seems that John is sick today.
b. John seems to be sick today.

My informants frequently felt that there was subtle difference between sentences like (24) and (25) but there was no systematic way of characterizing this difference. In all situations where one sentence could be used, the other could be used just as well.

Returning to sentence (24) we find that it is formed by the successive cyclic application of Emphasis Movement as illustrated by the sequence in (28):

(28) a. rkaž [$_S$ če xwain ldua7] ati abel
b. (25) rkaž [$_S$ xwain če ldua7] ati abel
c. (24) xwain rkaž [$_S$ če ldua7] ati abel

Emphasis Movement must apply to (28a.) moving xwain in front of the verb in the complement sentence giving (28b.). On the next cycle it can optionally apply extracting xwain from the complement S and placing it in front of the verb in the root S giving (28c.).

A somewhat more complex example of the successive cyclic application of the Emphasis Movement rule is sentence (29) which is derived by the sequence illustrated in (30):

(29) xwain guniž gira get gudo ne laž na ni gudož get ati xwain
Juan said-he all tortilla ate and he is who ate-he tortilla not Juan
'He said that Juan ate all the tortillas and he is the one who ate the the tortillas not Juan.'

(30) a. guniž [$_S$ gudo xwain gira get] ne laž na ni gudož get ati xwain
b. guniž [$_S$ xwain gudo gira get] ne laž na ni gudož get ati xwain

---

(footnote 6 continued)
the V and NP are immediately dominated by the same S. In addition, a two-rule proposal requires two nearly identical rules to be in the grammar of Zapotec. A grammar with one Emphasis Movement rule is obviously simpler. In footnote 7, we indicate that a further condition on the optional application of Emphasis Movement will be required. Even with this condition there are no advantages to the two-rule proposal. The further condition can be included in (26) with no more difficulty than it can be included in a separate Emphasis Movement rule.

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ON THE NECESSITY OF SEMIORDERED RULES[*]

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Introduction

There are three logically possible hypotheses about the use of extrinsic ordering in a grammar: 1) all rules are extrinsically ordered, 2) no rules are extrinsically ordered, and 3) some (but not all) rules are extrinsically ordered with respect to each other (semiordering). Examination of responses to acceptability judgments by both linguists and non-linguists demonstrated the existence of three groups of speakers: 1) those whose grammars allow Passive and Oblique Equi-NP Deletion to apply in only one order, 2) those whose grammars allow Passive and Oblique Equi-NP Deletion to apply as if unordered with respect to each other, and 3) those whose grammars could apply Passive and Oblique Equi-NP Deletion either in an ordered or unordered fashion. Assuming a lower-S analysis of adverbial clauses, neither strict ordering nor strict unordering of rules is a tenable hypothesis regarding the organization of all grammars. Grammars must be allowed to have both ordered and unordered rules. Assuming a higher-S analysis of adverbial clauses, strict unordering cannot be maintained, but both strict extrinsic ordering and semiordering are tenable. The higher-S analysis requires grammatical theory to permit lowering rules.

Although it is recognized in principle by generative grammarians that grammars are semiordered, most analyses dealing with rule order generally do not consider the unordered case when attempting to assert the extrinsic ordering of a rule.[1] It is generally agreed that at least some rules must be unordered in a grammar. For example, Ross (1969) has proposed that there be a rule of Pruning which simplifies phrase markers whenever its structural description is met. Nevertheless, Pruning has at least implicitly been treated as a special case. 'Ordinary' rules have generally been treated as being extrinsically ordered.

Ringen (1972) has pointed out that in a theory permitting unordered rules, obligatory rules are intrinsically ordered before optional rules with the same structural index. Thus, in such a theory there is no need to impose an extrinsic ordering between Reflexive (which is obligatory) and Extraposition (which is optional). Koutsoudas (1972) has further argued that contrary to Lakoff (1968: 81) Equi-NP Deletion and It-Replacement need not be strictly ordered as in (1), but must be unordered.

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[*] This work was begun at the University of Texas at Austin and was completed at SWRL. I would like to thank Emmon Bach, Stanley Peters and Harvey Rosenbaum for their criticisms and suggestions.

[1] See Koutsoudas, Sanders, and Noll, 1971 for a discussion with respect to phonology. See Koutsoudas, 1972; Ringen, 1972 and Lehmann, 1972 for syntactic examples.

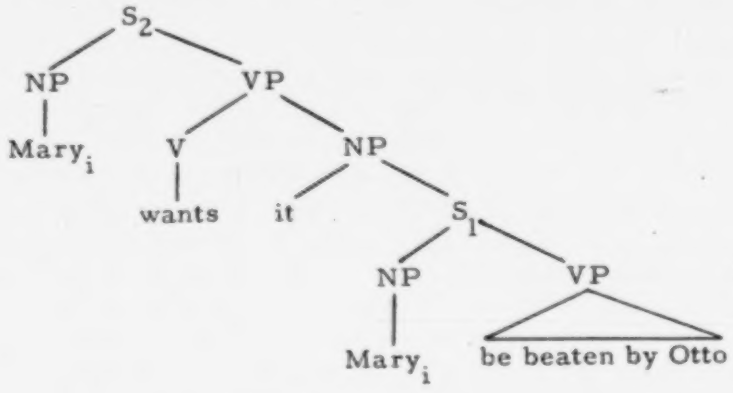
(1) a. Equi-NP Deletion
    b. It-Replacement

If these two transformations are ordered with respect to each other, it is impossible to derive both sentence (2) and sentence (3).

(2) Mary wants to be beaten by Otto.
(3) Mary wants herself to be beaten by Otto.

Koutsoudas argues that starting from (4), Lakoff's underlying structure for (2), and adopting ordering (1), it is not possible to derive (3).[2]

(4)



Facts such as these lead Koutsoudas to conclude that the strict ordering hypothesis is untenable. He notes that either semiordering or complete unordering are the only two alternative hypotheses.

Just as strict ordering can be falsified by showing one pair of rules which must be unordered with respect to each other, complete unordering can be falsified by showing one pair of rules which must be strictly ordered with respect to each other. Since there is no way of disproving the semiordering hypothesis, but a straight-forward means of disproving the complete unordering hypothesis, Koutsoudas adopts the latter as a working hypothesis.

Assuming a lower-S analysis for adverbial clauses, clear cases of the necessity of both extrinsic ordering and nonordering of a pair of rules can be found in the interaction of the Oblique Equi-NP Deletion Rule and Passive.[3]

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[2] This argument is not completely convincing because of the possibility that Mary in S_1 is marked as [+ Emphasis]. If this is the case, the two instances of Mary may lack sufficient identity for Equi-NP Deletion to apply, while having sufficient identity to satisfy the structural description of Reflexive.

[3] The key assumption of the lower-S analysis of adverbial clauses is that at the time that Passive first has the opportunity to apply to the main clause of sentences such as (5), the adverbial clause introduced by while is embedded in the main clause.

The analysis which follows does not depend on the precise formulation of the rule or rules creating passives. All that need be assumed is that some rule (which we will call Passive) places a non-agent NP to the left of the underlying subject NP.

Thus, data concerning the interaction of these two rules can be used to demonstrate the necessity of allowing grammars to have both extrinsically ordered and unordered rules.

Sentences such as (6) are clearly derived from the same underlying structures as sentences such as (5) by a rule deleting the subject of the embedded sentence.

(5) The people saw Sophia Loren while they were enjoying themselves.
(6) The people saw Sophia Loren while enjoying themselves.

For purposes of the following discussion we may assume that the copula is deleted by the same rule which deletes the subject. This rule of Oblique Equi-NP Deletion only operates when the deleted NP is identical to a NP in the matrix sentence. For most people the higher NP must be the subject of the matrix sentence, but some speakers can delete under identity with the direct object NP of the matrix sentence as well.[4] Thus, while almost all speakers accept (6), only a minority will accept (7).

(7) The people saw Sophia Loren while enjoying herself.

Only speakers who manifest a subject identity constraint by rejecting (7) provide crucial evidence on the order of application of Passive and Oblique Equi-NP Deletion.

Assuming extrinsic ordering, if Passive applies before Oblique Equi-NP Deletion, we would expect (8) to be acceptable and (9) to be unacceptable to speakers who reject (7).

(8) Sophia Loren was seen by the people while enjoying herself.
(9) Sophia Loren was seen by the people while enjoying themselves.

Similarly, if Passive is ordered after Oblique Equi-NP Deletion, speakers who reject (7) should accept (9) but reject (8).

The nonordering hypothesis on the other hand predicts that Passive can apply either before or after Oblique Equi-NP Deletion. Hence, it predicts that speakers who reject (7) will accept both sentence (8) and sentence (9).

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[4] Preliminary results indicate that these speakers can delete under identity with the subject, direct object or indirect object of the matrix sentence, but not under identity with other NPs. This is consistent with the claim that simple sentences consist of a verb with one to three NP arguments.

Ross (1972) has proposed a rule of Stuffing which optionally Chomsky adjoins ing to the verb of a relative clause without requiring an auxiliary be before the verb. His analysis has the advantage of providing a source for ings on stative verbs which cannot appear with ing in main clauses. If we adopt Stuffing, it is not necessary to assume that Oblique Equi-NP Deletion deletes instances of be as well as subjects. It is also possible to argue that the were of (5) is inserted by a transformational rule à la Bach (1967) which operates only when the subject noun phrase is present.

Study 1

Twenty-six graduate students and faculty members at the University of Texas at Austin were asked to give acceptability judgments to sentences similar to examples (6) - (9). The judgments were collected on an individual basis with the interviewer recording the responses. The sentences were repeated if requested.

Results of Study 1

As can be seen from Table 1, the informants can be divided into three main groups. The first seven informants accepted all four of the sentences, giving us no information on rule ordering. Informants 8 through 13 rejected the sentence corresponding to sentence (7) and accepted the remaining sentences. These speakers must have Passive and Oblique Equi-NP Deletion unordered with respect to each other. Informants 14 through 24, however, accept only the sentences corresponding to (6) and (8). To account for these speakers, it is necessary to posit grammars having Passive extrinsically ordered before Oblique Equi-NP Deletion.

To insist that all grammatical rules be extrinsically ordered with respect to each other would lead us to an ordering paradox in the description of speakers 8 through 13. To insist, on the other hand, that no rules may be extrinsically ordered with respect to each other would lead to a complication in the description of the grammars of speakers 14 through 24.

The adoption of the semiordering hypothesis that grammars can contain both extrinsically ordered and unordered rules, allows us to account for both of these two groups of speakers as well as to describe the difference between them in a natural way. Informants 1 through 24 all share an Equi-NP Deletion rule which applies to while-clauses with the constraint that the deleted NP be identical to some NP in the matrix sentence.[5]

Informants 8 through 13 have the additional constraint on Oblique Equi-NP Deletion that requires the matrix NP be the subject of the matrix sentence at the time that deletion occurs. Informants 14 through 24 share these two constraints and add a third: Oblique Equi-NP Deletion is ordered after Passive.

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[5] Strictly speaking this constraint is consistent with the current data but is not necessarily entailed by it.

Table 1

Study 1: Responses to Sentences with _While_-Clauses

Informant No.	Sentence					
	1	2	3	4		
1.	+	+	+	+	N	1 constraint a. NP = NP_{matrix}
2.	+	+	+	+	L	
3.	+	+	+	+	N	
4.	+	+	+	+	N	
5.	+	+	+	+	N	
6.	+	+	+	+	N	
7.	+	+	+	+	N	
8.	+	+	+	-	N	2 constraints a. NP = NP_{matrix} b. NP_{matrix} = subject
9.	+	+	+	-	N	
10.	+	+	+	-	N	
11.	+	+	+	-	L	
12.	+	+	+	-	L	
13.	+	+	+	-	L	
14.	+	+	-	-	N	3 constraints a. NP = NP_{matrix} b. NP_{matrix} = subject c. Passive precedes Equi-NP Deletion
15.	+	+	-	-	L	
16.	+	+	-	-	N	
17.	+	+	-	-	L	
18.	+	+	-	-	N	
19.	+	+	-	-	N	
20.	+	+	-	-	N	
21.	+	+	-	-	N	
22.	+	+	-	-	N	
23.	+	+	-	-	L	
24.	+	+	-	-	L	
25.	-	+	-	+	N	
26.	+	-	-	-	N	

Key to sentences:

1. We saw Sophia Loren while enjoying ourselves.
2. Sophia Loren was seen by us while enjoying herself.
3. Sophia Loren was seen by us while enjoying ourselves.
4. We saw Sophia Loren while enjoying herself.

N = Non-Linguist
L = Linguist

Study 2

A second study was designed to replicate and extend the results of Study 1. In addition to sentences using *saw* as the main verb and *enjoying* as the verb in the embedded sentence, a set of sentences was constructed using *watched* and *amusing*. Study 1 did not address the question of the relation between preposed and postposed *while*-clauses in sentences such as those in examples (11) and (12).

(11) While amusing ourselves, we watched Lyndon Johnson.
(12) We watched Lyndon Johnson while amusing ourselves.

These sentences undoubtedly have very closely related transformational histories. It is most natural to assume that sentences with preposed adverbial clauses are derived from the same underlying structures as sentences with postposed clauses, and that a rule of adverb preposing moves adverbial clauses to sentence initial position. If Oblique Equi-NP Deletion applies before Adverb Preposing we would expect the constraints on deletion in preposed and postposed adverbial clauses to be identical for a given individual. Note that Oblique Equi-NP Deletion can be instrinsically ordered before Adverb Preposing if we adopt a structural index utilizing the left-to-right ordering of the symbols as in (13).

(13) $[_S\ \underset{1}{X}\ \ \underset{2}{NP_1}\ \ \underset{3}{X}\ [_S\ \underset{4}{\underline{while}}\ \ \underset{5}{NP_1}\ \ \underset{6}{X]}\ \ \underset{7}{X]}$

There is no need to assume extrinsic ordering, for if Adverb Preposing were to apply before Oblique Equi-NP Deletion, the conditions for the application of the deletion rules would be destroyed. If, on the other hand, a rule were adopted which uses some other technique for identifying the identical NP in the matrix sentence, that is not dependent on the left-to-right ordering of the string (e.g., a distance principle), it would be possible to create a situation in which Oblique Equi-NP Deletion applies after Adverb Preposing and the appropriate 'model' NP which governs deletion varies. For speakers who have Oblique Equi-NP Deletion extrinsically ordered after Passive, we would expect to see reversals in the acceptability judgments between sentences with preposed and sentences with postposed while-clauses.

The sentences in Study 2 (see Tables 2 and 3) were presented to a group of 20 psychology students at the University of Texas who had had no linguistic training. The interviewer read the sentences twice unless requested to repeat, and the informants recorded their own judgments on answer sheets.

Results of Study 2

The response of 13 of the 20 informants to the sentences involving *saw* showed the same three patterns as the majority of the informants from Study 1 (see Table 2). There is no apparent theoretically motivated explanation for the responses of 6 of the remaining seven informants.

can be seen from Table 3, 16 of the 20 informants exhibited the same three main patterns for sentences involving *watched* and *amusing* as were found for sentences with saw and enjoying. One of the remaining informants (17) seems to have the same constraints on Oblique Equi-NP Deletion as the other speakers who accept only two sentences except that Oblique Equi-NP Deletion is ordered before Passive. The patterns of the remaining three informants have no theoretically motivated explanation.

Somewhat surprisingly, the constraints on the sentences with watched seem to be more stringent than those on the sentences with saw (see Table 4). In particular, eight speakers who accepted the active saw-sentence with deletion under object identity (8) rejected the corresponding sentence with watched. It cannot be clear what should be used to account for this difference until a replication has been completed using a variety of lexical items.

As can be seen from Table 5, the same three basic patterns of acceptability exist for preposed while-clauses as for postposed clauses. This is particularly interesting in light of the fact that sentences with preposed adverbials and deletion under nonsubject identity (such as (14)) are explicitly rejected by the school grammars, while the corresponding sentences with postposed adverbials (such as (15)) are generally ignored by these grammars.

(14) While amusing ourselves, Lyndon Johnson was watched by us.
(15) Lyndon Johnson was watched by us while amusing ourselves.

Informants who do not give identical responses to corresponding sentences with preposed and postposed clauses show a marked preference for the sentences with preposed clauses (13:6). This fact is currently without explanation.

Although there is considerably more variation in the data of Study 2 than was found in Study 1, the same three basic patterns of responses occur in both studies. The patterns for preposed and postposed sentences are highly similar, lending support to the hypothesis that Oblique Equi-NP Deletion is not extrinsically ordered with respect to Adverb Preposing but is intrinsically ordered before it. An unexplained difference was found between the responses to sentences involving saw and those involving watched. Deletion under object identity with an active matrix sentence was more readily accepted in the former than in the latter. Preposed while-clauses are somewhat more acceptable than postposed while-clauses.

Table 2

Study 2: While-clauses with saw as the matrix verb

Informant No.	Sentence				
	1	2	3	4	
1.	+	+	+	+	1 constraint a. NP = NP_{matrix}
2.	+	+	+	+	
3.	+	+	+	+	
4.	+	+	+	+	
5.	+	+	+	+	
6.	+	+	+	+	
7.	+	+	-	+	No theoretically motivated explanation.
8.	+	+	-	+	
9.	+	+	-	+	
10.	+	+	+	-	2 constraints a. NP = NP_{matrix} b. NP_{matrix} = subject
11.	+	+	+	-	
12.	+	+	+	-	
13.	+	+	+	-	
14.	+	+	-	-	3 constraints a/b. NP = NP_{matrix} = subject c. Passive precedes Equi-NP Deletion
15.	+	+	-	-	
16.	+	+	-	-	
17.	+	-	-	+	
18.	-	+	-	-	No theoretically motivated explanation.
19.	+	-	-	-	
20.	+	-	-	-	

Key to sentences:

1. We saw Sophia Loren while enjoying ourselves.
2. Sophia Loren was seen by us while enjoying herself.
3. Sophia Loren was seen by us while enjoying ourselves.
4. We saw Sophia Loren while enjoying herself.

Table 3

Study 2: *While*-caluses with *watched* as the matrix verb

Informant No.	Sentence 1	2	3	4	
1.	+	+	+	+	1 constraint a. $NP = NP_{matrix}$
3.	+	+	+	+	
4.	+	+	+	-	2 constraint a. $NP = NP_{matrix}$ b. NP_{matrix} = subject
13.	+	+	+	-	
5.	+	+	+	-	
14.	+	+	+	-	
2.	+	+	+	-	
20.	+	+	+	-	
17.	+	-	+	-	3 constraints (?) a. $NP = NP_{matrix}$ b. NP_{matrix} = subject c. Equi NP precedes Passive
11.	-	+	+	-	*
12.	+	+	-	-	3 constraints a. $NP = NP_{matrix}$ b. NP_{matrix} = subject c. Passive precedes Equi-NP Deletion
9.	+	+	-	-	
19.	+	+	-	-	
15.	+	+	-	-	
7.	+	+	-	-	
16.	+	+	-	-	
18.	+	+	-	-	
6.	+	+	-	-	
10.	-	+	-	-	*
8.	-	+	-	-	

Key to sentences:

1. We watched Lyndon Johnson while amusing ourselves.
2. Lyndon Johnson was watched by us while amusing himself.
3. Lyndon Johnson was watched by us while amusing ourselves.
4. We watched Lyndon Johnson while amusing himself.

---

*No theoretically motivated explanation.

Table 4

Comparison of sentences with saw and watched as main verb

	Saw				Watched			
	Subject Agreement		Non-Subject Agreement		Subject Agreement		Non-Subject Agreement	
Informant number	active	passive	passive	active	active	passive	passive	active
1.	+	+	+	+	+	+	+	+
2.	+	+	+	+	+	+	+	-
3.	+	+	+	+	+	+	+	-
5.	+	+	+	+	+	+	+	-
4.	+	+	+	+	+	+	-	-
6.	+	+	+	+	+	+	-	-
7.	+	+	-	+	+	+	+	+
8.	+	+	-	+	-	+	-	-
9.	+	+	-	+	+	+	-	-
10.	+	+	+	-	+	+	-	-
11.	+	+	+	-	+	+	-	-
12.	+	+	+	-	+	-	+	-
13.	+	+	+	-	-	+	-	-
15.	+	+	-	-	+	+	+	-
14.	+	+	-	-	+	+	-	-
16.	+	+	-	-	+	+	-	-
17.	+	-	-	+	+	+	-	-
18.	-	+	-	-	+	+	+	-
19.	+	-	-	-	-	+	+	-
20.	+	-	-	-	+	+	+	-
Number of Acceptances	19	17	10	10	17	19	10	2

		watched	
		+	-
saw	+	39	17
	-	9	15

Table 5

Comparison of preposed and postposed *while*-clauses

	Preposed Reflexives				Postposed Reflexives			
	Subject Agreement		Non-Subject Agreement		Subject Agreement		Non-Subject Agreement	
Informant number	active	passive	passive	active	active	passive	passive	active
1.	+	+	+	+	+	+	+	+
3.	+	+	+	+	+	+	-	-
7.	+	+	+	+	+	+	-	-
6.	+	+	+	+	+	+	+	-
4.	+	+	+	-	+	+	+	-
8.	+	+	+	-	+	+	-	-
13.	+	+	+	-	+	+	+	-
17.	+	+	+	-	+	+	-	-
18.	+	+	+	-	+	-	+	-
2.	+	+	+	-	+	+	+	-
9.	+	+	+	-	-	+	-	-
12.	+	+	-	+	+	+	+	+
15.	+	+	-	+	-	+	+	-
5.	+	+	-	-	+	+	+	-
11.	+	+	-	-	+	+	-	-
16.	+	+	-	-	+	+	-	-
19.	+	+	-	-	-	+	-	-
10.	+	+	-	-	+	+	-	-
14.	+	-	-	-	+	+	-	-
20.	+	-	-	-	+	+	+	-
Number of Acceptances	20	18	11	6	17	19	10	2

		Postposed	
		+	-
Preposed	+	42	13
	-	6	19

Study 3

Study 3 was designed to replicate the results of Studies 1 and 2 with respect to postposed while-clauses but in a written mode of presentation. The four sentences of interest were randomly embedded among 67 other sentences dealing with relative clauses.[6] These sentences were presented to the informants in writing along with the following instructions:

> The following sentences vary in acceptability for different speakers of English. Please indicate your initial reactions according to the scale on the next page. Please do not: (1) change your first reaction, (2) discuss these with anyone else, (3) reread sentences which you have already marked.

The materials were returned at the informants' convenience. Of the 27 graduate students and faculty from various educational institutions in the Los Angeles area who completed the questionnaire, 21 had a significant amount of linguistic training.

Results of Study 3

Nineteen of the informants responded with patterns identical to the three main patterns found in the previous studies (see Table 6). Informant twelve's pattern could be accounted for by a grammar having three constraints on Oblique Equi-NP Deletion:

1. The deleted NP must be identical to some NP in the matrix sentence.
2. The antecedent NP in the matrix sentence must be the subject of its sentence.
3. Oblique Equi-NP Deletion is extrinsically ordered before Passive.

Informant twenty-three's responses suggest a grammar like the above with the second constraint changed so that the model NP is the object of the matrix sentence. The remaining 6 informants' responses have no theoretically motivated explanation. These results correspond closely to the results of the other two studies.

It is worth noting that only one of the six non-linguists in Study 3 responded with one of the three main patterns found in all the studies. This fact suggests that at least in the written mode, linguists and non-linguists are approaching the task of assigning acceptability judgments differently. This difference can probably be attributed to the difficulty of making the nature of the acceptability judgment clear to a naive informant. In the case of the orally presented materials, informants were permitted to ask for clarification of the instructions.[7] This opportunity was generally not available to the informants who responded to the written sentences.

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[6] Study 3 was embedded in a larger study reported in part by Elliott, Legum, and Thompson (1969).

[7] Usually a phrase like 'would you ever say this sentence given the appropriate circumstances' seemed to convey the nature of the task well.

Table 6

Study 3: Responses to Sentences with While-clauses

Informant number	Sentences					
	1	2	3	4		
1.	+	+	+	+	N	1 constraint a. $NP = NP_{matrix}$
2.	+	+	+	+	L	
3.	+	+	+	+	L	
4.	+	+	+	+	L	
5.	+	+	-	+	L	*
6.	+	+	+	-	L	2 constraint a. $NP = NP_{matrix}$ b. NP_{matrix} = subject
7.	+	+	+	-	L	
8.	+	+	+	-	L	
9.	+	+	+	-	L	
10.	+	+	+	-	L	
11.	-	+	+	-	N	*
12.	+	-	+	-	N	3 constraints, including Equi-NP Deletion ordered before Passive
13.	+	+	-	-	L	3 constraints, including Equi-NP Deletion ordered after Passive
14.	+	+	-	-	L	
15.	+	+	-	-	L	
16.	+	+	-	-	L	
17.	+	+	-	-	L	
18.	+	+	-	-	L	
19.	+	+	-	-	L	
20.	+	+	-	-	L	
21.	+	+	-	-	L	
22.	+	+	-	-	L	
23.	-	+	-	+	N	3 constraints, including Equi-NP Deletion ordered before Passive and NP_{matrix} = object
24.	+	-	-	-	L	*
25.	-	+	-	-	L	
26.	-	-	+	-	N	
27.	-	-	-	-	N	

Note:
L = Linguist
N = Non-Linguist

Key to sentences:
1. The people saw Sophia Loren while enjoying themselves.
2. Sophia Loren was seen by the people while enjoying herself.
3. Judy was seen by the people while enjoying themselves.
4. The people saw Karen while enjoying herself.

---

* No theoretically motivated explanation.

Discussion

Of the 16 logically possible patterns of responses to the four paradigm sentences with postposed *while*-clauses, 12 appear in the data from the three studies (see Table 7). Over three quarters of the response patterns, however, were concentrated in the three most frequent patterns. These three patterns can all be accounted for in theoretically natural and interesting ways. Three of the remaining patterns (representing 9% of the total) can also be described in grammatically meaningful terms. It is not clear, however, that it is necessary or reasonable to do so in light of the small number of informants exhibiting the patterns. The remaining 6 patterns make up 12% of the data, but have no theoretically reasonable explanations which can be associated with them. The existence of these patterns is probably due to the relatively high level of 'noise' which is associated with collecting acceptability judgments. This noise has at least three sources: (1) the difficulty of disambiguating the question "Is this sentence acceptable to you?" (2) the indeterminacy of introspection, and (3) the increasing difficulty of making consistent judgments after 10 to 20 sentences have been processed.

Two main differences between linguists and non-linguists are apparent from a comparison of their responses to the postposed *while*-clauses:

1. The linguists tended to have more constraints on the Oblique Equi-NP Deletion rule than the non-linguists,

and

2. The linguists' responses tended to be cleaner; i.e. fewer non-explainable patterns appeared in the data from linguists.

Conclusion

The existence of three dominant sets of response patterns to acceptability judgments about sentences having undergone Oblique Equi-NP Deletion from *while*-clauses has been demonstrated with three different modes of data collection. If we adopt a lower-S analysis for adverbial clauses in order to explain these patterns it is necessary that grammatical theory allow grammars to utilize both extrinsically ordered and unordered rules. Since the extrinsic ordering of two rules limits the application of one or both of the rules it is reasonable to conceive extrinsic ordering as a set of constraints on rule application. Two rules can be assumed to be unordered with respect to each other unless one of them contains an explicit constraint ordering it before or after the other. If rule ordering is conceived of in this fashion, it is necessary to reconsider much of recent linguistic analysis. In particular, it is necessary to reevaluate those analyses which depend crucially on the assumption that all rules are extrinsically ordered.

Furthermore, it is no longer necessary to insist that children acquire extrinsic ordering along with each rule. Indeed, the data examined above are consistent with the hypothesis that the basic outline of a rule is acquired first and that the constraints on the rule can be acquired later. In the case

Table 7

Comparison of Response Patterns of Linguists and Non-Linguists in Studies 1, 2, and 3

	Sentence Type				Percentage of Informants Responding with Pattern		
	1	2	3	4	Non-Linguists (n = 44)	Linguists (n = 29)	Total (n = 73)
Major patterns having theoretical explanations	+	+	+	+	30	14	23
	+	+	+	-	16	28	21
	+	+	-	-	23	48	33
Minor patterns having theoretical explanations	+	+	-	+	7	3	5
	+	-	+	-	2	0	1
	-	+	-	+	5	0	3
Patterns without theoretically motivated explanations	-	+	+	-	2	0	1
	+	-	-	+	2	0	1
	+	-	-	-	7	3	5
	-	+	-	-	2	3	3
	-	-	+	-	2	0	1
	-	-	-	-	2	0	1

Key to sentences:

1. We saw Sophia Loren while enjoying ourselves.
2. Sophia Loren was seen by us while enjoying herself.
3a. Sophia Loren was seen by us while enjoying ourselves.
b. Judy was seen by the people while enjoying themselves.*
4a. We saw Sophia Loren while enjoying herself.
b. The people saw Karen while enjoying herself.*

---

*Sentences 3b. and 4b. were used in Study 3.

of Oblique Equi-NP Deletion, one of these constraints can be that it follow Passive. Since speakers frequently have the subject identity constraint but not the ordering constraint on Oblique Equi-NP Deletion, and since the converse occurs only rarely, it is reasonable to assume that the extrinsic ordering constraint on Oblique Equi-NP Deletion is acquired relatively late if at all. If the hypothesis that rule ordering constraints are acquired after the rules to which they apply is accurate, it should be possible to actually observe the ontogeny of this feature.

These conclusions follow only if we adopt the lower-S analysis for adverbial clauses. It is possible to reject the lower-S analysis in favor of a higher-S analysis: an analysis in which adverbial clauses are in higher sentences at the time that Passive first has the opportunity to apply to the surface main clauses. If we adopt a higher-S analysis for adverbial clauses, it is necessary that the third major pattern of Table 7 (++--) have Passive ordered before Oblique Equi-NP Deletion. The second major pattern of Table 7 (+++-) can be analyzed either as having Oblique Equi-NP Deletion ordered before Passive or as having these two rules unordered. The reason for this is that for each of the three possible orderings — Passive then Deletion, Deletion then Passive, or unordered — Passive will always have the chance to apply to the surface main clause before Oblique Equi-NP Deletion has a chance to apply to the entire sentence.

Indeed, for Oblique Equi-NP Deletion ever to get the opportunity to apply before Passive, it is necessary (1) that we posit a rule that lowers adverbial clauses into the surface main clause and (2) that we allow Passive a second chance to apply to this transformed main clause. If we do not allow grammars to do both these things, it will not be possible to write a grammar for speakers of pattern 2 (+++-).

Allowing both of these innovations, we can write a grammar for the third pattern (++--) by ordering Passive before Oblique Equi-NP Deletion. We can write a grammar for pattern 2 (+++-) either by ordering Oblique Equi-NP Deletion before Passive or by leaving the two rules unordered. Both of these options allow both orders of rule application.

There is an interesting distinction in the predictions of possible grammars made between the lower-S analysis of adverbial clauses and the higher-S analysis. The lower-S analysis predicts that it is possible to have a grammar which produces the second minor pattern in Table 7 (+-+-): a grammar with a subject identity constraint and Oblique Equi-NP Deletion always applied before Passive. The higher-S analysis predicts that no such grammar is possible. At the moment it is an open question whether the two speakers who responded with this pattern did so in error, or whether one or both of them in fact has just such a grammar.

To summarize then: If we adopt a lower-S analysis of adverbial clauses, then our grammatical theory cannot insist on either strictly ordered or strictly unordered grammars. If we adopt a higher-S analysis of adverbial clauses, grammatical theory cannot allow strictly unordered grammars, but must allow lowering rules.

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ON RAISING*

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On the basis of evidence presented by Rosenbaum (1967), Ross (1967), G. Lakoff (1971), R. Lakoff (1968), Stockwell et al. (1973), Kiparsky and Kiparsky (1968) and Postal (to appear), I will assume that there exists a rule of RAISING in the grammar of English. That this rule is not meaning-preserving is a well-known fact which has been repeatedly noted in the literature (see, for example, Jackendoff, 1969; Partee, 1968, 1971; Cantrall, 1970; Chomsky, 1970; Shopen, 1972; and Postal, to appear). Amongst those who recognize a rule of RAISING, most do not go beyond a simple statement that, in their view, the semantic contrasts induced by the transformation should be handled by derived-structure interpretive rules. Postal is the only one who goes into a little detail. He suggests what he calls an "assumption-linkage" analysis. He discusses examples like the following:

(1) a. Julius Caesar struck me as honest.
    b. It struck me that Julius Caesar was honest.

(2) a. It just now struck me that my wife has been dead two years tomorrow.
    b. *My wife just now struck me as having been dead two years tomorrow. (From Cantrall, 1970)

This is what Postal says about them:

> "Assuming that the term Julius Caesar designates the well-known Roman figure, there is no context in which, for example, the present writer can really use (4)a. (4)b, on the other hand, could naturally be utilized in a discussion of Rome, famous Romans, etc.
>
> The contextual contrasts are, I claim, a function of differences in meaning. I cannot use (4)a because to do implies (i) that I have in fact had some kind of perceptual experience of Julius Ceasar, and (ii) the judgment expressed is a function of this experience, and (i) is false so that (ii) also is. Notice that (4)a would become natural again in a context of time, travel, etc. On the other hand, (4)b is normal in ordinary discourse because it makes no assumption about the speaker's experience vis-a-vis Julius Caesar. Observe also that (i) and (ii) explain what is wrong with Cantrall's example (10), since the judgment expressed could not have been based on perceptual experience with the entity designated by the raised NP (under normal assumptions about dead people being buried, etc.).[1]

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[*] I wish to express my appreciation to Paul Schachter and Sandra A. Thompson for reading the manuscript and offering valuable comments and constructive criticism.

[1] For (4) read my (1); for "example 10" read my (2).

The application of RAISING, then, according to Postal, causes just those meaning differences which exist between the a.- and the b.- sentences in (1) and (2). In (1a.) we assume that the speaker makes his assertion on the basis of personal experience with the individual designated by the subject NP, whereas the b.- sentence is not associated with such an assumption.

I feel that Postal's semantic intuitions about the meaning of the two strike-sentences is correct. He goes on to suggest, however, that what we have to do with here is a special feature of natural languages, namely, the linkage of certain assumptions with certain rule applications. He explicitly states that in his view these assumptions are "more properly regarded as linked with RAISING than as part of the meaning of strike." His arguments consist in showing that the assumptions are not under the scope of explicit negations, questioning, or higher predicates. For example, if we say

(3) Peter did not strike me as honest.

we do not negate the linked assumption that we base our judgment on direct evidence gained through perceptual experience with Peter. Similarly, for (4b.,c.):

(4) a. Peter struck me as honest.
    b. Did Peter strike you as honest?
    c. I believe Peter struck you as honest.

Questioning (4a.) does not question the associated assumption, nor does embedding place the assumption under the scope of the higher predicate. To support his view that the assumptions associated with the 'raised' structures are linked with the rule application, and not with the main verb, Postal offers the following examples with the verb find which, I agree, exhibit similar meaning differences to those found with the strike-sentences:

(5) a. I found Julius Caesar (to be) boring.
    b. I found that Julius Caesar was boring.

In short, Postal's position appears to be that the meaning differences are linked ("in some yet technically unspecified way," to be sure), with RAISING and are not part of the meaning of the main verb. Later, in the course of his discussion of the verb allow, he makes the following statement, which seems to contradict his previous claim: "Thus, it seems to me that the aspect of the meaning of allow I have designated Q does pattern like P with strike."[2] In view of this inconsistency in his exposition, it is no longer clear what Postal's position really is, and I will therefore make no further comment on his proposal at this time. Instead, I will present a brief but systematic survey of the meaning contrasts induced by raising operations and will argue that these meaning contrasts are dependent upon the main verb and the nature of the raised NP, and not upon the transformation itself.

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[2] Q is the (optional) assumption associated with sentences like I allowed Bob to leave, namely that the subject of allow expressed his permission to the individual designated by the raised NP. P is the assumption associated with the strike-sentences.

I will offer a tentative solution to the problem within the framework of the Extended Standard Theory.

Consider, to begin with, the sentences in (6) and (7) which are generally regarded as related by the rule of Tough Movement:

(6) a. It is difficult to handle this child.
b. This child is difficult to handle.

(7) a. It is easy to play sonatas on this violin.
b. Sonatas are easy to play on this violin.
c. This violin is easy to play sonatas on.

The sentences in each set are not entirely synonymous. In (6a.) *difficult* is predicated of the entire complement clause, whereas in (6b.) the basic predication seems to have changed insofar as the difficulty of handling *this child* now seems to be attributed to some inherent property of the derived subject. The sentence now receives an additional interpretation parallel to that of a simple sentence like "This child is naughty," which does not, however, destroy the understood deep relations between the verb *handle* and *this child*. We still understand the relation between *handle* and *this child* to be a verb-object relation. On the other hand, the relation between the main predicate *difficult* and *this child* is now felt to be much closer than in the a.-sentence. *This child* in b. occupies a position of semantic prominence which Chomsky (1965) describes in terms of the surface structure notion of 'topic.' *This child*, then, has become the 'topic' of the sentence, and the rest of the string is what is known as the 'comment.'

The sentences in (7), which have been attributed to Klima, and which are discussed in Chomsky, 1970; Partee, 1968,1971; Jackendoff, 1969,1972; and G. Lakoff, 1970 also appear to differ in topic. Note that Partee and Jackendoff speak about the meaning differences between the sentences in (7) as having to do in some vague sense with focus.[3] I do, however, feel that the term 'topic' is more appropriate here. Be that as it may, the main observation that needs to be made about these three sentences is that they differ in meaning in that (a) is an assertion about the entire complement, (b) makes an assertion about *sonatas*, and (c) makes an assertion about *this violin*. Once again we notice that the understood verb-object relation between *play*, *sonatas*, and *violin* remains unaffected by the surface manifestations of the three sentences. Within the framework we have adopted here, the sentences do not have to differ in deep structure.

In sum, the main semantic difference sensed between the "raised" *easy/difficult* sentences and their non-raised counterparts can, I believe, be described in terms of the notion of 'topic.' This characterization holds for all the predicates which allow Tough Movement, as illustrated by a number of additional examples:

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[3] It is not clear in which sense the word 'focus' is used by them.

(8) a. It is inappropriate for Jane to read this book.
b. This book is inappropriate for Jane to read.

(9) a. It is impossible to solve this puzzle.
b. This puzzle is impossible to solve.

(10) a. It is a pleasure for Mary to do the dishes.
b. The dishes are a pleasure for Mary to do.

Sentences with Tough Movement predicates, then, exhibit the kind of correlation between the transformation and the meaning change which would support Postal's hypothesis. It should be noted, however, that Postal (1971) himself expressed some doubts as to the existence of his Tough Movement rule. In a prefatory note to his Crossover Phenomena he suggested that sentence pairs like (6) should be derived from separate underlying structures. If his views on the subject have not changed, then sentences like (6) do not support his hypothesis, since they are not, according to this view, transformationally related. But, if we regard Tough Movement as a transformation, as I would be inclined to do, then it would indeed seem that the meaning changes must be linked with the transformation. In the remaining part of the paper I will show that as soon as we look at the semantic effects of some of the other RAISING operations, this view cannot be upheld.

Consider first sentences which, I would claim, are transformationally related by a rule which raises the subject of the embedded sentence to main subject position (in Stockwell et al. this rule is called RAISE-subject-to-subject). Consider, to begin with, sentences (11) below:

(11) a. It is certain that nobody will pass the exam.
b. Nobody is certain to pass the exam.

These sentences are discussed in Partee, 1971, where it is pointed out that they are sharply non-synonymous, even though they seem to differ syntactically only in that in b. RAISING has applied. I want to exclude sentences containing quantifiers from our discussion of RAISING, since the meaning differences cannot be attributed to the raising operation. They are due to the difference in scope of the negative quantifier nobody. That this is so becomes quite clear from analogous sentences without quantifiers:

(12) a. It is certain that Peter will pass the exam.
b. Peter is certain to pass the exam.

which are near paraphrases of each other.

Jackendoff (1969) argued that contrasts like those in (11) follow from a general principle relating to the scope of quantifiers in a sentence, namely, left-to-right precedence in surface structure correlates with wider scope in logical structure. To illustrate the generality of this principle, consider

(13) a. Nothing is difficult for Jack to undertake.
b. It is difficult for Jack to undertake nothing.

which are also non-synonymous.

Let us now turn to sentences (14):

(14) a. It appears that John is shooting Bill.
b. John appears to be shooting Bill.
c. Bill appears to be being short at by John. (Partee)

According to Partee, the semantic contrast in the appear-sentences seems to involve a difference in point of view of the speaker. In the a.-sentence he is taking in the whole situation, whereas in b. and c. he 'focusses' on John and Bill respectively. The difference is one of manner of perception, what part of the sentence the speaker wishes to give semantic prominence, and not of the basic meaning. Notice that this is not the only meaning difference between the appear-sentences with infinitival complements and their that-clause analogues. There is an obvious similarity between the interpretation of the strike-sentences and the appear-sentences under discussion. Sentences (14b, c.) imply that the speaker actually sees the event taking place. The a.-sentence, on the other hand, would be perfectly appropriate in a context where neither John nor Bill are within the speaker's field of vision. With both verbs, then, the raised sentences are associated with the assumption of perceptual experience with the individual designated by the raised NP. We must now ask whether this connection between the two sets of sentences is due to the raising rule itself, or to the obvious similarity in meaning between the two verbs strike and appear. My claim is that the linked assumptions are verb-dependent, and not a function of the raising rule. To show this, we need to give an example with a predicate that allows RAISE-subject-to-subject, but for which the raised sentence does not exhibit the meaning contrasts found with strike and appear. Such a predicate is certain. Consider sentences (12) which we have claimed are related by the rule of RAISE-subject-to-subject. Notice that (12b.) does not imply that I arrived at the judgment expressed as a result of personal acquaintance with Peter. I can think of situations in which I need never have met or seen Peter in order to assert that he is certain to pass his exam.

My hypothesis then is that the meaning differences under discussion are verb-dependent, that is, that we get one kind of interpretation with some verbs, and another kind with others. If my claim is correct, i.e., that the assumptions are part of the meaning potential of verbs, then this information must be made available for semantic interpretation in the lexical entry for verbs. A verb like appear, or strike, therefore, must contain in its lexical entry two kinds of information: a syntactic part describing the form of the sentence in which it can appear, and a semantic part in the form of a derived structure interpretation rule which is somehow linked with the syntactic information.

So far we have given evidence that the assumptions in question are verb-dependent. We will now show that they are also dependent upon the nature of the raised NP. Notice that appear-sentences with there and 'whether it' subjects do not differ in interpretation from their that-clause versions:

(15) a. It appears that there is much trouble in the Middle East.
b. There appears to be much trouble in the Middle East.

(16) a. It appears that it is raining in the mountains.
b. It appears to be raining in the mountains.

Now notice that sentences with idioms have no linked assumptions either:

(17) a. It appears that advantage was taken of his ignorance.
b. Advantage appears to have been taken of his ignorance.

Such facts suggest that appear-sentences are only linked with assumptions when the raised NP can be interpreted referentially. To account for this the derived structure interpretive rules must be made sensitive to the nature of the derived subject. Note that this "assumption-linkage" is sometimes obligatory, and sometimes optional. For appear it seems not to be obligatory, as evidenced by (18):

(18) John appears to be unhappy.

Such a sentence can be used in a situation where the speaker does not actually see John, for example, when he is reporting on what someone else has said about John. It seems to be the case then, that appear-sentences with raised subjects that are something "perceptible" are at least potentially ambiguous as between an "assumption-linked" interpretation and one that is not. In the case of strike, however, the assumption-linkage is obligatory. Strike, as is well-known, does not allow raising with there, weather it, or idiom chunks:

(19) a. *There struck me as being much trouble in the Middle East.
b. *It struck me as raining in the mountains.
c. *Heed strikes me as being taken of his warning.

Recall that strike-sentences like (1) were said to be associated with the assumption that the speaker has had perceptual experience with the entity designated by the raised NP. This, of course, is not possible in sentences (19), where the raised NP is non-referential. As Postal correctly pointed out, sentences like (19) can be explained in terms of violations of the condition that the raised NP must be something "perceptible," if the assumption-linkage for strike is made obligatory.

In summary, we have observed that sentences with verbs like certain, appear, and strike (usually assumed to allow RAISE-subject-to-subject) exhibit certain meaning contrasts between their infinitival and their that-clause versions. The semantic difference between the certain-sentences seemed to be one of focus,[4] while the appear- and strike-sentences were associated with certain assumptions concerning the speaker's perceptual experience with the referent of the main subject NP. Furthermore, the assumptions were optional in the appear-cases, and obligatory with the strike-sentences. I have argued that the associated assumptions are verb-dependent, and not a function of the raising rule per se, as Postal would have it. I have also shown that the assumption-linkage depends further on the nature of the raised NP. I suggested that the phenomenon be explained in terms of interpretive rules which are optional for some verbs and obligatory for others, and that this be part of the semantic information marked in the lexical entry for verbs.

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[4] I am using the term 'focus' in the sense defined by Jakobson (1966) to refer to the concentration of attention or interest, and not in the sense used in Chomsky, 1970 where it is defined in terms of stress on surface structure constituents.

Let us now turn to sentences arising through the application of the RAISE-subject-to-object rule. Consider first the verb believe, which must be a raising verb because of sentences like (20):

(20) I believe there to be much trouble in the Middle East.

where there can certainly not be regarded as the deep object of believe. Now consider the following examples from Partee's recent paper "The semantics of belief-sentences":

(21) a. Tom believes that Cicero denounced Catiline.
b. Tom believes Cicero to have denounced Catiline.

In her discussion of these sentences, Partee quotes Quine's (1969:145-50) judgment that the two sentences in (21) are not synonymous, in particular, that only in the b.-sentences is Cicero in purely referential position. She suggests that Quine's semantic intuition may be wrong, since believe also occurs in sentences like (22), where there and it cannot be taken as referential.

(22) a. Tom believes there to have been an earthquake recently.
b. Susan believes it to be likely that no one will show up.

Partee herself is inclined to regard (21b.) ambiguous, with a slightly greater tendency for Cicero to be regarded as referential there than in the a.-sentence. She then offers the following example, which she says is definitely ambiguous.

(23) John believes a Communist to have been at the heart of the plot.

This, in turn, she says, again argues against Quine's interpretation. Let us look at several more examples:

(24) a. Mary believes that she is beautiful.
b. Mary believes herself to be beautiful.

(25) a. Mary believes that John is a liar.
b. Mary believes John to be a liar.

The difference in meaning in the last two sets seems to me to have much more to do with the focus in the Jakobsonian sense than with reference. The raised noun phrase acquires a certain measure of semantic prominence by being moved closer to the main verb.[5] The semantic contrast with believe-sentences, however, is very subtle, and there is even no general agreement whether one exists at all. But notice that for those who can discern a meaning-difference, it only appears with objects that can be understood referentially.

The meaning contrasts induced by RAISING are again different with a verb like expect. Expect is usually analyzed as taking a whole sentence for its direct object in contrast to persuade which has an NP object plus a sentential complement. The reasons for this analysis have to do with contrasts arising when the embedded sentence is passivized (cf. Rosenbaum, 1967). Such an analysis is consonant with the semantic intuition that expect expresses a binary relation between an individual and an anticipated event. Expect must be a raising verb because of sentences like

(26) a. I expected there to be much trouble.
b. I expected it to rain today.
c. I expected heed to be taken of his warning.

Consider the sentences below:

(27) a. I expect that John will take Mary to the movies.
b. I expect John to take Mary to the movies.
c. I expect Mary to be taken to the movies by John.

Shopen (1972) noticed that the sentences in (27) are not synonymous. He claims that the meaning difference between the a.-sentence and the b.- and c.-sentences amounts to something like expecting it of the individual designated by the derived object noun phrase to bring about the anticipated event described in the complement clause. In the b.-sentence, he says, I expect the event to be brought about by John, while in the c.-sentence I seem to hold Mary responsible for the occurrence of the event.[6] If this is indeed the case, then we have further evidence for our claim that the interpretation of the complement depends upon the main predicate.

My position, then, is that the information relating to possible meaning contrasts induced by RAISING should be available in the lexical entry for main predicates. A promising formalism for expressing this information in lexical entries is presented in Shopen (1972). Part of the material that

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[5] In Korean, the semantic import of a constituent is measured in terms of its proximity to the main verb. In general, new information is placed as close as possible to the verb. Hungarian is somewhat similar in this respect. Whenever a major consitituent is singled out by the speaker, he will place it before the verb. Note that both Korean and Hungarian are case-marking languages with fairly free word order.

[6] For most people I have asked this difference seems more difficult to get with c. than with b. The reason is that for most people the person of which one can expect something must be at least capable of doing it, which means that the NP following expect must be interpretable as an AGENT, which is not the case in a passive sentence like c.

Shopen would include in lexical entries for verbs corresponds exactly to what Postal calls the "assumptions linked with the RAISING application."

Finally, I would like to raise the question whether _strike_ is a raising verb at all, i.e., whether there really is a transformational relationship between the sentences in (28).

(28) a. It struck me that Peter was honest.
b. Peter struck me as honest.

Notice that RAISING normally leads to an infinitival complement, and not to an _as_-complement as taken for granted in Postal's examples. But the sentence with the infinitival complement is ungrammatical:

(29) a. *Peter struck me to have been honest.
b. Peter struck me as (having been) honest.

Furthermore, unlike _seem_ and _appear_, which are definitely "raising" verbs, _strike_ does not allow existential _there_ weather _it_ or idiom chunks in subject position (cf. 30-32). There are also further differences as illustrated in (33):

(30) a. There appeared to be much trouble in the Middle East.
b. *There struck me to be much trouble in the Middle East.

(31) a. It appeared to be too late to go shopping.
b. *It struck me to be too late to go shopping.

(32) a. Heed seemed to be taken of his warning.
b. *Heed struck me to be taken of his warning.

(33) a. John appears to be passing by.
b. *John strikes me to be passing by.

Considering _strike_ a raising verb would require at least the following conditions on the transformation:

(i) _Strike_ only allows RAISING with _-ing_ complements. (e.g. It struck me as being too late to go shopping.)

(ii) _Strike_ does not "raise" existential _there_, weather _it_, or idiom parts. (cf. examples (30-32)).

(iii) _Strike_ only allows RAISING if the embedded predicate is an adjective (e.g. Mary strikes me as being too stupid for words. *Tim strikes me as being a bricklayer. *John strikes me as being passing by.)

(iv) RAISING is necessarily linked with _as_-insertion.

A transformation inserting _as_ would be required just for this one predicate and seems to be otherwise unmotivated in the grammar. The facts cited above seem to go against considering _strike_ as a raising predicate. It would be much simpler to consider strike as an _as_-complement taking predicate, say like _regard_ (e.g. I regard him as incompetent) which is not a subject-raising verb in anyone's grammar, and to subcategorize _strike_ both as a subject-embedding verb, and an _as_-complement-taking verb.

2

The facts above seem to argue very strongly that the relation between sentences like those in (28) is not transformational. But if *strike* does not allow RAISING, then the meaning contrasts discussed cannot be linked with the transformation.

Postal can only handle the semantics of his *strike* sentences on the assumption that they are related by the RAISING transformation. The solution I am proposing is independent of the transformation and can handle the semantics of such sentence pairs whether they are transformationally related or not.

On the basis of evidence presented, I conclude that RAISING induces semantic contrasts which are a function not of the rule itself, but of the main predicate and the raised NP, and that the semantic contrasts are best handled by semantic interpretation rules operating on the output of the transformation.

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NATURAL FOCUS AND AGENTIVE INTERPRETATION: ON THE SEMANTICS OF DUTCH EXPLETIVE *er*[*]

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I shall use the term *focus* in a Jakobsonian sense (1966: 269-270) to refer to the concentration of attention or interest. Consider sentences (1):

(1) a. It appears that John is shooting at Bill.
b. John appears to be shooting at Bill.
c. Bill appears to be being shot at by John.

Barbara Partee has pointed out that the difference between these sentences is a difference in the speaker's point of view (1971: 17): " . . . -- in a. he is taking in the whole situation, in b. and c. he is *focussing on* [emphasis mine, RSK] John and Bill respectively." In other words, in (1b, c.) John and Bill are in some sense *in focus*. Our attention in each case is centered or focussed on one individual to the exclusion of the other.

Now it has long been recognized that the beginning of the sentence is a position of *natural discourse focus* -- a position of prominence reserved, by and large, for important items. One of the most interesting aspects of Modern Dutch grammar is the use of the weak deictic word *er*, 'there,' to remove indefinite subject noun phrases from this position of prominence. Consider sentences (2):

(2) a. Een hond blaft. [GENERIC] and [VIVID NARRATIVE]
'A dog barks.'

b. Er blaft een hond. [LEISURELY NARRATIVE]
there barks a dog
'A dog barks.'

In the a. sentence, the noun phrase is in discourse focus in initial position. In the b. sentence, the weak deictic *er* occupies initial position and the subject noun phrase is, as it were, "out of focus" behind the verb. It is the aim of this paper to show that the use of *er* has an important semantic consequence. Specifically, in rather subtle ways, it *decreases* the possibility of interpreting the subject noun phrase as "agent-like." This "deagentivizing" is the consequence of two factors which I shall now discuss: (i) the *defocussing* effect of *er*, and (ii) the relation between focus and agency.

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The general effect er has in sentences (2) may be summarized as follows. In unemphatic word order in Dutch, it is typically given information rather than new information which is put into discourse focus at the beginning of the sentence. This is a reasonable communicative strategy, for it gives the hearer a kind of "orientation" to the new material. It also makes sense in terms of focus, for focus -- a concentration of attention -- requires that there be something that attention can be centered on. It will be more difficult to center attention on something totally unfamiliar than on something known. Accordingly, we would expect that, by and large, definite constituents -- referring to something well-defined -- would more compatible with discourse focus than would indefinite constituents. The use of er in (2b.) may then be viewed in part as a way of getting an indefinite constituent out of the focus of initial position -- where it is somewhat incoherent -- while still keeping the verb second where it will not be taken as signaling a condition or question. (In declarative Dutch sentences the verb always comes second.) Observe that sentence (2b.), in which een hond is out of focus, has but one interpretation: leisurely narrative. It announces -- without emphasis -- the appearance on the scene of a specific dog. Sentence (2a.), in contrast, with een hond in focus, has two interpretations. In the generic reading, een hond is in focus because -- though indefinite -- 'a dog' is nonetheless to be taken as relatively given rather than relatively new. If the referent -- 'a dog' -- is unfamiliar (the claim of the indefinite article) but still more given than it might be (the claim of the word order), then perhaps the specific identity of the dog is unimportant, whence the generic reading of 'any dog.' The second interpretation of (2a.) -- vivid narrative, the sudden appearance on the scene of a particular dog -- is, in its turn, the consequence of the use of discourse focus for "shock value." Specific indefinites do not normally belong in focus. By putting one there, we draw special attention to it. We thrust it, so to speak, into the center of the arena. Both the generic and the vivid narrative reading of (2a.) are thus consistent with the discourse focus of initial position. And the leisurely narrative reading of (2b.) is consistent with the defocussing effect of er. In summary, the interpretations given the subject noun phrase in (2) correlate in each case with the degree of focus on that noun phrase.

For the sake of completeness, we may note that the weak deictic er can not normally be used to defocus a definite subject. In (3a.) the noun phrase de hond -- in either generic or narrative reading -- refers to something identified or given. To claim, as in (3b.) that the given 'dog' is also new is a contradiction:[1]

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[1] The reason for this is that er is so weak a deictic that it can refer only to identified spatio-temporal situations. To claim that, in a familiar setting, something given is also new is, of course, contradictory. However, if the entire situation is unfamiliar, then even a given participant involved in it can be taken as new. Sentences such as Daar blafte de hond! 'There! The dog barked!' containing the strong deictic daar, are perfectly coherent. For discussion, see Kirsner (1972: 419-421).

(3) a. De hond blaft. [GENERIC] and [NARRATIVE]
'The dog barks.'

b. *Er blaft de hond.
'There barks the dog.'

Turning now to the relation between focus and agency, I believe that there is a great deal of evidence that the more agent-like a participant is, the more worthy it is of attention, the more likely it is to be focussed on. First of all, in many languages, it is typically the agent noun phrase rather than the non-agents which the morphology characterizes as the so-called subject, or *participant in focus in the event named by the verb*. Passive sentences -- focussing on a *non*-agent -- remain rare in normal discourse. For example, in written Dutch, a passive occurs on the average only once every five pages or so.[2] And in Jan Svartvik's extensive work on English (1966: 50-51), passives made up only 12% of the data. The relative infrequency of passives, then, suggests that speakers by and large regard agents as more important than non-agents.

A second reflex of the greater importance or "focus-worthiness" of agent noun phrases is their greater tendency to be definite. We would of course expect that participants in focus would tend to be identified sooner than participants out of focus. To pick an analogy from life in general, i.e. from non-linguistic human behavior, we learn the names of important people (spouses, bosses) and don't bother to learn the names of unimportant people (janitors, mailmen). Accordingly, if agents are indeed more important, we would expect the noun phrases referring to them to be more definite. Two kinds of evidence suggest that this is, in fact, so. First of all, there is the indirect evidence provided by languages in which direct objects -- but not subjects -- may bear a distinctive morphology. For example, Hebrew and Turkish mark definite direct objects, but not indefinite ones.[3] And in Spanish, the preposition *a* is used almost exclusively to mark definite animate objects, especially humans. It is tempting to think that these morphological markers are -- or once were -- "anti-ambiguity devices" used to prevent objects from being confused with subjects.[4] The fact that it is definite and animate objects which are marked suggests that it is these objects which are most similar to subjects and, hence, that agents are -- on the whole -- more definite (and, of course, more animate) than non-agents.

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[2] Scherps (1966), cited in Langendonck (1968).

[3] See Rosén (1962: 65), Lewis (1955: 27), Thomas and Itzkowitz (1967: 36).

[4] Consider the contrast between the Spanish sentences *Se mató el prisionero.* 'The prisoner killed himself.' where the third person singular verb is taken as referring to *el prisionero*, and *Se mató al* [*al* = *a* + *el*] *prisionero.* 'One/They killed the prisoner.' where *prisionero* is *not* taken as the subject of *mató*.

More direct evidence for the definiteness of agents is provided by counts of the relative frequency of personal pronouns as subject and object in languages where verbs have an explicit subject. For example, in a recent study of Modern English,[5] 59% of subjects but only 24% of objects were personal pronouns. Since personal pronouns are inherently definite, the frequencies support the claim. The greater statistical definiteness of agents would then be reasonable evidence for their greater importance.

A third indication of the greater importance of agents is word order -- specifically, Greenberg's observation (1966) that agents tend to precede non-agents, i.e. to occur at or near the beginning of the sentence. Striking additional confirmation of the greater "focus-worthiness" of agents is given by peculiar restrictions on the Navajo phenomenon of subject-object inversion. The basic declarative word order in Navajo is SOV, but if the verb is marked with a special prefix, the object may precede. However, according to Kenneth Hale (1970) and Mary Taptto (1971), Navajo ranks nouns on a absolute, event-independent scale of relative agent-likeness or, in Hale's words, "relative potency." Given two nouns of unequal rank, the higher or more agent-like noun must always precede, whether or not it is subject or object in the particular event described in the sentence at hand.[6] Consider sentences (4) below.[7] Man ranks higher than baby on the Navajo scale of agent-likeness. Accordingly, the noun man must always come first. Therefore, if the man kicks the baby, the "active" sentence (4a.) is possible, but not the "passive" sentence (4b.). If the baby kicks the man, only the "passive" sentence (4d.) is possible, and not the "active" sentence (4c.):

(4) a. Diné 'awéé'chí'í yiztał.
man baby yi-kicked
'The man kicked the baby.'

b. *'Awéé'chí'í diné biztał.
baby man bi-kicked.
'The baby was kicked by the man.'

c. *'Awéé'chí'í diné yiztał.
baby man yi-kicked.
'The baby kicked the man.'

d. Diné 'awéé'chí'i biztał.
man baby bi-kicked.
'The man was kicked by the baby.'

In Hale's own words, the effect of the ordering is "to give prominence to, to put into focus [emphasis mine, RSK]" the higher ranking or more potent noun phrase.[8] The Navajo case thus lends further support to the claim that the more "agentive" a participant, the more likely it is to be focussed on.

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[5] Aarts (1971: 282-283 and 286-287).

[6] However, see Hale (1970: 20) for an indication that there may be "secondary reorderings," provided no ambiguity results.

[7] Taptto (1971).

[8] Hale (1971: 10).

To recapitulate, I have cited three kinds of evidence to support the claim of the greater "focus-worthiness" of agents: (i) the low frequency of passive sentences, centering attention on non-agents, (ii) the greater frequency of definite agent noun phrases as opposed to definite non-agent noun phrases, and (iii) the fact that agents rather than non-agents are overwhelmingly placed closer to the beginning of the sentence, in a position of discourse focus.

Now in view of the preceding discussion about both *er* and the relationship between focus and agency, I believe we can make the following prediction:

(5) The argument (stated informally)

Premise 1: NP is agent-like IMPLIES NP is in discourse focus.
Premise 2: *Er*-introduced NP IMPLIES NP is not in discourse focus.
- -
Conclusion: *Er*-introduced NP IMPLIES NP is not agent-like.

If agent-like noun phrases are normally put in discourse focus and if *er* is used to defocus subject noun phrases, to -- in Jespersen's words (1965:154) -- "hide away the subject and reduce it to an inferior position," then, by a simple logical equivalence, *er*-introduced indefinite subject noun phrases should, in some sense, be *less agent-like* than non-*er*-introduced indefinite subjects. I shall now present several kinds of evidence that this is indeed the case.

Consider first of all the possibility of using expletive *er* in active transitive sentences containing two or more participant noun phrases, i.e. sentences with an explicit subject and explicit objects. Unlike English, Dutch does indeed permit such sentences, as is shown by (6):

(6) a. Een jongen heeft een steen gegooid.
a boy has a stone thrown
'A boy threw a stone.'

b. Er heeft een jongen een steen gegooid.
there has a boy a stone thrown
'It happened that a boy threw a stone.'[9]

Note, however, the subtle contrast. The a. sentence, much more direct, is sometimes characterized as depicting an action (*handeling*), while the b. sentence, indirect, is sometimes said to describe a situation or state (*toestand*).[10] This suggests two lines of attack. First, we might look at the semantics of individual verbs. The more *action*-like the event depicted by nouns and verb, the more *agent*-like the subject, and the less coherent *er* should be. Sentences (7) below contain the verb *besluipen*, 'to steal upon, to come over,' a verb which can have either an animate or an inanimate subject:

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[9] This gloss was suggested to me by Prof. Seeger Bonebakker. Compare also Es and Caspel (1972:146), where a paraphrase of the form *Het geval heeft zich voorgedaan dat*..., 'The event occurred that...,' is proposed.

[10] Nieuwborg (1968:202).

(7) a. Een commando besloop hem.
'A commando snuck up on him.'

b. Een gevoel van angst besloop hem.
'A feeling of anxiety came over him.'

In (7a.) the commando is an active agent -- actively creeping up on the victim, preparing to attack. In (7b.), however, the subject is not an active agent but merely an emotion perceived. We might predict, then, that since the commando is more of an agent, *er* would be less coherent in (7a.) than in (7b.). This is indeed the way it turns out; native speakers uniformly accept (8b.) but reject (8a.):

(8) a. *Er besloop hem een commando.
'There snuck up on him a commando.'

b. Er besloop hem een gevoel van angst.
'There came over him a feeling of anxiety.'

A variation on the lexical theme is to keep the same topic -- in this case, emotional states -- but to change the verb. Consider sentences (9). In (9a.) -- the same as (7b.) -- the perceiver is merely the locus of the emotion. In (9b.), in contrast, the perceiver is a *victim.* He not only feels the emotion; he is crushed by it:

(9) a. Een gevoel van angst besloop hem.
'A feeling of anxiety came over him.'

Een gevoel van angst overweldigde hem.
'A feeling of anxiety overwhelmed him.'

Relative to the object *hem,* 'him,' the anxiety is much more "potent" in the b. sentence than in the a. sentence. Accordingly, we might predict that while *er* could be used in the a. sentence, it would be incoherent in the b. sentence. This is exactly what we observe. While (10a.) is coherent, (10b.) is soundly rejected by all informants:

(10) a. Er besloop hem een gevoel van angst.
'There came over him a feeling of anxiety.'

b. *Er overweldigde hem een gevoel van angst.
'There overwhelmed him a feeling of anxiety.'

To recapitulate, the coherence of (8b.) and (10a.), where the subject is *not* a true agent, and the incoherence of (8a.) and (10b.), where the subjects *are* more agent-like, supports our claim that *er*-introduced subjects are *less* "potent."

The second line of attack on transitive sentences suggested by the labels of "action" and "state" is quantitative. Agents are, of course, most reasonably agents of *actions*, not of states. If, in active sentences, subjects are more agent-like than objects, and if agents "deserve" focus, and if *er* defocusses subjects, then *er*-introduced multi-participant sentences should be less coherent than plain multi-participant sentences. Now given that language is used for communication, we would generally expect coherent sentences to occur in discourse more often than incoherent sentences. Consequently, if *er*-introduced transitive sentences are self-contradictory, we would expect them to be avoided. Hence, we might predict that the percentage of *er*-introduced sentences with two or more participants would be less than the percentage of plain sentences with two or more participants. Table I, below, summarizes the result of counts made on several corpora of the relative frequency of one-, two-, and three-participant sentences with different kinds of subject noun phrases, namely: definite subjects, indefinite subjects, and indefinite subjects introduced by *er*.[11] Ignoring the three-participant column, where differences are negligible, we observe that in each corpus and in the summary, two-participant sentences constituted about 17% of sentences with plain indefinite subjects, but only 1% of sentences with *er*-introduced indefinite subjects. The *er*-introduced two-participant sentences are very infrequent, and our hypothesis is supported.[12]

TABLE I

NUMBER OF PARTICIPANTS PER SENTENCE
AS A FUNCTION OF SUBJECT FOCUS

Subject Type	%1 - P sentences	%2 - P sentences	%3 - P sentences	Total	N
	Corpus:	Haasse, *De Meermin*			
Def.	54	44	2	100	287
Ind.	84	15	1	100	149
Er.. Ind.	99	1	0	100	120
	Corpus:	Van het Reve, *De Avonden*, I			
Def.	48	50	2	100	50
Ind.	83	17	0	100	23
Er.. Ind.	100	0	0	100	18
	Corpus:	Van het Reve, *De Avonden*, IV			
Def.	49	49	2	100	39
Ind.	74	26	0	100	19
Er.. Ind.	100	0	0	100	7
	Corpus:	Hermans, *Behouden Huis*			
Def.	65	34	1	100	71
Ind.	78	22	0	100	59
Er.. Ind.	100	0	0	100	52
	Corpus:	Hermans, *Donkere Kamer*			
Er.. Ind. ONLY	99	1	0	100	398

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[11] See Kirsner (1972: 437-443) for more details. For discussion and examples of quantitative validation of linguistic analyses, see Diver (1969), especially pp. 57-59 and pp. 62-67, and Zubin (1972), Chapters 3, 4 and 5.

[12] The figures also provide evidence for the general correlation of focus and agency discussed above. Note that the percentage of multi-participant sentences is highest for definite subjects, i.e. those subject most coherent with discourse focus.

SUMMARY

Subject Type	% 1 - P sentences	% 2 - P sentences	% 3 - P sentences	Total	N
Def.	54	44	2	100	447
Ind.	82	17	1	100	250
Er.. Ind.	99	1	0	100	595

A third type of evidence that *er*-introduced subjects are less agent-like is again quantitative, but is given by one-participant sentences alone. Here we have counted the frequency of animate indefinite subjects as a function of the use of *er*. The rationale is as follows.

Generally speaking, agent-like noun phrases are animate and non-agent-like noun phrases are inanimate. For example, in a count of the first fifty two-participant sentences in each of three separate Dutch corpora, 94% of the subjects were animate, but only 25% of the objects. If use of *er* to defocus a subject is inconsistent with agentive interpreatation, then the percentage of animate indefinite subjects should be lower in *er*-introduced sentences than in plain sentences. Table II gives the figures obtained from four separate corpora:[13]

TABLE II

SUBJECT ANIMACY AS A FUNCTION OF SUBJECT FOCUS IN ONE-PARTICIPANT SENTENCES (INDEFINITE SUBJECTS ONLY)

Subject Type	Number Inanimate	Number Animate	Total	% Animate Subject NP
	Corpus:	Hellinger, *Vlammen*		
Ind.	60	25	85	29
Er .. Ind.	86	19	105	18
	Corpus:	Campert, *Nietsnut*		
Ind.	10	20	30	67
Er .. Ind.	42	22	64	34
	Corpus:	Van het Reve, *Haringgraten*		
Ind.	2	3	5	60
Er .. Ind.	15	5	20	25
	Corpus:	Haasse, *Meermin*		
Ind.	71	29	100	29
Er .. Ind.	41	9	50	18
		SUMMARY		
Ind.	143	77	220	35
Er .. Ind.	184	55	239	23

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[13] See Kirsner (1972: 444-448) for more details.

Note that, in each corpus and in the overall summary, the relative frequency of animates in the plain sentences is about one and a half times that in the *er*-introduced sentences. The hypothesis is thus supported. Acutally, the figures are better than they seem here, for I have not separated out those sentences without *er* in which a locative phrase comes first, i.e. sentences like *In de tuin blafte een hond.* 'In the garden barked a dog.' Consider now Table III, where the separation is made:

TABLE III
SUBJECT ANIMACY AS FUNCTION OF SUBJECT FOCUS
IN ONE-PARTICIPANT SENTENCES (INDEFINITE SUBJECTS ONLY),
WITH LOCATIVE-INITIAL SENTENCES SEPARATED

Subject Type	Number Inanimate	Number Animate	Total	Animate NP
Ind. - first	44	42	86	49
Loc. . Ind.	99	35	134	26
Er . . Ind.	184	55	239	23

Observe that indefinite subjects in discourse focus are animate roughly 50% of the time, but that indefinite subjects out of discourse focus are animate only 25% of the time. Since the use of expletive *er* is one way of defocussing the subject, and since animates are more likely to be agents than are inanimates, these data support the claim that *er*-introduced indefinite subjects are indeed less agent-life.

To summarize, I have presented several kinds of evidence that use of expletive *er* to remove indefinite subjects from the beginning of the sentence also decreases the possibility that these subjects will be agent-like. The reason for this would seem to be that speakers find agents inherently more important or more interesting than non-agents. If agent-like noun phrases "belong" in discourse focus, then to put a subject noun phrase *out of focus* is to imply that the subject is not really an agent after all. In this context, it is instructive to point out that the same tendencies we have found in Dutch also turn up, as we might expect, in related languages. German, for example, has a so-called expletive *es* which is roughly comparable to Dutch *er*. Its use in two-participant sentences (quite frequent a few centuries ago) is severely restricted.[14] The same holds true for Danish expletive *der*.[15] And in English, use of expletive *there* in active transitive sentences such as (11) died out in the sixteenth century:[16]

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[14] Whereas a German sentence like (i) does turn up in formal speech (for example, in radio announcements of concerts), sentence (ii) is considered unacceptable:

(i) Es spielt jetzt das Berliner Philharmonische Orchester die Fünfte Symphonie von Beethoven.
'There plays now the Berlin Philharmonic Orchestra Beethoven's Fifth Symphony.'

(ii) *Es hat ein Junge ein Fenster eingeworfen.
'There smashed a boy a window.'

Thanks go to Peter Schmidt and Hans Wagener for discussing these and other German sentences with me.

(11) *There killed a cop a hippie.

Furthermore, if we look at those one-participant *there*-sentences which do occur in English, we note that in none of them is the subject noun phrase agent-like. For example, in (12a.) we have a banner waving passively in the wind and in (12b.) we have a person actively waving his hand. *There* can be used as in (12c.), but it would be totally absurd in (12d.):

(12) a. A tattered banner waved from the flagpole.
[Non-agentive subject, good sentence in descriptions.]

b. A bearded student waved from the roof.
[Agentive subject]

c. There waved from the flagpole a tattered banner.

d. *There waved from the roof a bearded student.

And whereas in (13a.), the soldiers might be agents of the act of remaining, they cannot be so in (13b.):

(13) a. Five soldiers remained in the camp.

b. There remained five soldiers in the camp.

It then follows that volitional adverbs will be coherent in the former sentence, but not in the latter:

(14) a. Five soldiers $\left\{\begin{array}{l}\text{voluntarily}\\ \text{cleverly}\\ \text{stubbornly}\end{array}\right\}$ remained in the camp.

b. ?There $\left\{\begin{array}{l}\text{voluntarily}\\ \text{cleverly}\\ \text{stubbornly}\end{array}\right\}$ remained five soldiers in the camp.

The facts about German *es*, Danish *der*, and English *there* thus lend further support to our claim that defocussing a subject noun phrase also "deagentivizes" it. We can account for the phenomena on the basis of the principle that speakers are more interested in agents than in non-agents.

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[15] Consider the following Danish sentences provided by James R. Massengale:

(i) Der gøede en hund.
'There barked a dog.'

(ii) Der løb en hund efter en kat.
'There ran a dog after a cat.'

(iii) *Der bed en hund en kat.
'There bit a dog a cat.'

[16] See Visser (1963: 52). The non-occurrence of this type of *there*-sentence was noted by Perlmutter (1970: 116), but no attempt was made to explain it.

Agents "belong" in focus. We have here, then, an interesting case in which the explanation for a body of linguistic data has followed not from purely formal syntactic considerations but rather from a reasonable assumption about the interests of speakers.

The theoretical implications of this are, I think, very much worth stressing. Though it is a truism that language is a device of communication used by human beings, contemporary linguistics has paid remarkably little attention to what might be called the role of the "human factor" in grammar -- i.e. to language as a specific instance of human behavior. We have here attempted to redress the balance. In its widest terms, this paper deals with the interaction of two general communicative strategies, each reflecting certain psychological properties of people. The first strategy -- the placement of indefinite noun phrases out of focus, away from the beginning of the sentence -- undoubtedly reflects the relative difficulty of processing new information. The second strategy -- of putting agents in focus -- appears, in its turn, to reflect the greater interest people have in active rather than passive participants. We have seen that the result of the interaction -- in Dutch and other languages -- has been that the use of expletive particles to defocus indefinite noun phrases tends to imply that these noun phrases are not as agent-like as they might be. Now one specific consequence of this "deagentivization" is the disfavoring of expletives in transitive (multi-participant) sentences. In Dutch the disfavoring is still only a tendency; sentences such as (6b.) Er heeft een jongen een steen gegooid. 'It happened that a boy threw a stone.' are perfectly normal. But in Modern English (and Danish), where sentences like (11) *There killed a cop a hippie. are totally incoherent, one may indeed argue that the tendency has been "grammaticalized," that it has acutally become part of the structure of the language. It would seem, then, that if linguistic theory is to explain the basic facts of the organization and use of language, it must directly confront the "human factor." Linguistic theory must explicitly recognize the role of the interests and psychological properties of speakers in determining both language use and language structure.[17]

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[17] For further discussion of the role of the human factor in grammar, see García (forthcoming) and Zubin (1972: 100-107).

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CONSTRAINTS ON PHRASE STRUCTURE CONFIGURATIONS*

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1. The Structure-Preserving Constraint

The central problem considered in this study has preoccupied students of transformational grammar since its beginnings: the problem of imposing constraints on grammatical transformations. Much important recent work has been motivated by the same problem (Ross, 1967; Chomsky, 1972; Schwartz, 1972; and Bresnan, 1972, for example). With these investigators, I start from the premise that various conditions and restrictions on when various transformations may apply are not ad hoc specifications which are part of the individual rules, but are rather reflections of some deeper grammatical principles which define the formal framework in which transformational rules operate.

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*This paper is hopefully the next-to-last draft of the first chapter of a book, the rest of which will be a revision of my doctoral dissertation (Emonds, 1970), supplemented by articles that have appeared elsewhere (Emonds, 1972 and 1973). There will of course be changes in what appeared in the dissertation and in the other articles, but much of what was in these studies will remain substantively unchanged.

The purpose of this first chapter, besides defining my claims, is to answer either explicitly or indirectly criticisms; this, naturally, entails in some cases important changes in the definition of my formal framework and in analyses of particular constructions. I am of course much indebted to those who have made well thought out criticisms of my previous work. In particular, a large part of the chapter is given over to defining constraints on base rules which are crucial in defining a constraint on transformations which depends on the form of base rules.

Two important revisions of my dissertation are not included in this first chapter. One is a new analysis of WH-preposing which accounts for the data given in the dissertation that indicates that the rule is structure-preserving, and yet avoids generating phrase nodes in the COMP position in the base. The crucial step which allows this is a generalization of the "left-branch constraint" of Ross (1967) along the lines suggested in Banfield (1973b). This "extended left-branch constraint" permits WH-preposing to be stated without mention of a phrase node in its structural description. There are many steps of the argument that require independent justification, and space does not permit even tracing the argument in detail, much less justifying each step.

The second revision is in accord with a gentle counsel given me by Morris Halle: "Get rid of doubly-filled nodes." I am not sure I share the general distrust of this concept, particularly if one stipulates that transformations cannot create them, but in any case I have eliminated them. I have

Constraints on grammatical transformations can be defined and classified in various ways;[1] for our purposes, it may be helpful to consider the following subdivision. Certain constraints, including some proposed by Bresnan, Chomsky, Ross and Schwartz, essentially specify that constituents cannot be moved out of certain structural configurations, even though these configurations satisfy the structural description of a transformation that would otherwise move these constituents.

On the other hand, certain constraints proposed by Chomsky and Schwartz, for example, attempt to define what conditions are necessary in order for the structural change of a transformation to apply. The constraints defined in this study are of the same type; that is, they essentially specify that constituents cannot be moved into certain structural configurations.

From a study of English transformations, and some consideration of French syntactic processes (which has been greatly aided by consideration of the work of Ruwet (1972) and Kayne (1973)), I have arrived at a hypothesis for a general constraint on grammatical transformations, which may be termed the "structure-preserving constraint." Of course, the constraint is subject to testing, refinement, and perhaps drastic modifications on the basis of other languages, as we cannot expect inductions made on the basis of a few languages to survive in unmodified form when applied to a wide range of languages. (Unless there is in fact a great deal of formal similarity in the syntax of all natural languages.)

Some preliminary definitions are necessary for a succinct statement of the constraint.

Root Sentence. A root S is the highest S in a tree, or any S which is not dominated by, or a sister constituent to, any node other than S.

I follow Ross (1967) in analyzing coordinating conjunctions as being dominated in surface structure by the conjoined constituent they introduce. Hence, in a sentence consisting of coordinately conjoined S nodes, these nodes are root S's. In general, the notion of root S and the notion of independent clause,

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(* continued)
been able to do this by re-introducing the notion of "generalized transformation," less, however, the T-markers and Type-two Projection Rules of generative grammar predating Chomsky (1965). This re-introduction of generalized transformations also allows elimination of some very basic problems with the sentence-boundary filtering mechanism introduced in Chomsky (1965). Further, the treatment of complementation I will propose that uses generalized transformations both allows me to answer certain valid criticisms of my previous treatment offered in Higgins (1973) connected with sentence-pronominalization, topicalization, and sub-categorization, and to account for roughly the same range of facts accounted for in Kiparsky and Kiparsky (1971) without sacrificing any advantages of my own previous analysis.

[1] In particular, see Chomsky, 1972: 1-3.

taken from traditional grammar, are closely related. However, there are cases where a root S possibly arises by transformation from a clause which is dependent or even non-existent in deep structure. Some examples of such root S's (underlined):

Had John arrived earlier, everyone would have been less upset.
The first chapter had been censured, as had the introduction.
Will we be welcome, do you think?
The table goes with the lamp, doesn't it?
I guess the table goes with the lamp alright, doesn't it?

It would seem that the notion " independent clause" is therefore more closely related to the generative notion "deep structure root S" than to the more general term "root S."

It might be thought that special provision must be made in the definition of root S for the S nodes of direct quotation, which, as we will see, have all the properties of a root S. However, the most enlightening treatments of direct quotation are, in my opinion, those which, for reasons independent of those of interest here, treat the S nodes of direct quotation as separate sentences (or trees) of discourse in deep structure. (Cf. Partee, 1973, and Banfield, 1973). These authors claim that a direct quotation is an independent sentence of discourse, so that the sentence "Bill said, 'I am tired'." is derived from the discourse: "Bill said this: I am tired." In this two-sentence discourse , when this is anaphoric (i.e., does not refer to something outside the discourse, such as some writing on a paper held by the speaker), it unambiguously refers to the sentence following. In such an analysis, a rule is of course required which deletes this, and perhaps also attaches the quoted S as a sister to the introductory S, but I see no reason to assume that the quoted S is transformationally subordinated to (made a part of) the introductory clause.

Root Transformation. A transformation (or a transformational operation, in the case of a transformation performing several operations) which moves, copies, or inserts a node C into a position in which C is immediately dominated by a root S is a "root transformation" (or a root transformational operation).

Structure-Preserving Transformation. A transformation (or a transformational operation, in the case of a transformation performing several operations) which introduces no node C into a position in a phrase marker where the grammatical rules of the language do not independently generate C is called "structure-preserving."

That is, a transformational operation is not structure-preserving if it moves, copies, or inserts a node C into some position where C cannot be otherwise generated by the grammar. In particular, according to this definition, any deletion rule is structure-preserving.

Minor Transformation. A transformation which affects only a sequence of a single non-phrase node C and of one adjacent constituent C' which is specified without a variable, such that the rule is not subject to any condition nor has any effect exterior to C-C' (or C'-C). is called a "minor transformation."[2]

I place no special restriction on how a minor transformation may "affect" the sequence C-C': it may reorder them, or alter their dominance relations, or both; it may change a feature on C or C'; it may delete C or C', or it may be a rule to insert one in the context of the other. What is insisted on is the strictly local nature of a minor transformation, and the restriction that one of the two constituents involved not be a phrase node.

If a minor transformation reorders C and C', I will refer to it as a "minor movement transformation." Transformations that are not minor transformations will be called "major transformations."

Two well-known rules of English which can be written as minor movement transformations are the optional interchange of an object NP and a post-verbal particle and the obligatory reordering of a head adjective and the modifying intensifier (degree word) enough (cf. too big, so big, as big, but big enough). According to the above definition, such minor transformations can depend on the structural and lexical characteristics of the constituents interchanged, but not on other characteristics of the phrase markers to which the rules apply. Similarly, a rule which inverts a pre-verbal clitic sequence around the verb that depends only on conditions on the verb and on the clitic sequence is a minor movement transformation. (Such a rule can account for the position of clitics in Spanish, as the pre-verbal, post-verbal alternation appears to depend on whether the verb is +TENSE or not.)

On the other hand, a rule like subject-auxiliary inversion in standard English cannot be a minor transformation, because it is dependent on the presence in the string of a WH (or NEG) constituent at the beginning of the clause. The same remark holds for the inversion of the clitic pronoun subject around the verb in questions in French. A third case of interchange of adjacent constituents which cannot be a minor transformation is the placement of object clitics after the verb in affirmative imperatives in French, for this rule depends on the absence of any material in the string to the left of the clitics (except perhaps material set off by commas).

With these definitions, we can state the structure-preserving constraint in the form it will be defended in this study:

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[2] Phrase nodes are, roughly, nodes which dominate phrases rather than lexical items or grammatical formatives. I return below to an explicit characterization. The set of phrase nodes for English included S, NP, AP, VP, PP, and perhaps one, two, or three others, depending on the exact formal framework used. The choice of a formal framework is, of course, dependent on empirical adequacy.

Structure-Preserving Constraint. Major grammatical transformations must perform either root or structure-preserving operations.

Of course, the empirical claim embodied by this constraint is heavily dependent on the correct determination of the phrase structure (or base) rules for any given language, and on any general restrictions that can be placed on possible sets of base rules for any language. The main purpose of this study is to propose such restrictions.

Before stating these restrictions, however, I will answer an objection to the dependence of the structure-preserving constraint on the base rules that might be made no matter how severely the latter were restricted, and two other plausible general objections to the "sense" of the constraint.

Given that the formal method for stating structure-preserving rules used here often requires that such transformations substitute constituents for nodes which dominate only "dummy" or "empty" structure, one may ask, what is to prevent one from making any non-structure-preserving rule which moves category A into C___D into a structure-preserving one simply by including A as an optional expansion in the base rule generating C-D (i.e. by changing B---> . . . -C-D- . . . into B---> . . . -C-(A)-D- . . .). This criticism has been made most clearly in Hooper, 1973: 41-42 , her statement of it bears repetition in full:

> "In order for the SP constraint to make any strong claims about the nature of language or transformational rules, the PS rules must be independently motivated. All PS rules postulated must be necessary to generate surface word orders that cannot be shown to be derived by transformation. Emonds' hypothesis is weakened considerably by the postulation of nodes that are always empty in deep structure, because such nodes can have no independent justification, i.e. they cannot be shown to be necessary for generating surface word orders except through the application of SP transformations.
>
> We have already seen that the SP formulation of the passive transformations requires that an obligatory empty NP occur after agentive by. In this case the phrase structure required is well-motivated, because PP's do occur in this position in surface structure and there is no reason to believe that such PP's are placed by transformation. A more serious threat to Emonds' hypothesis is the postulation of nodes that are always empty and that occur in PS positions which are not independently necessary for generating surface structures. Three transformations discussed by Emonds utilize such obligatorily empty nodes: Indefinite Subject Movement, Clitic Placement (in Spanish and French), and Wh-preposing."

I believe that Hooper's criticism is well-taken (there is a discussion of this difficulty in Emonds (1970) in the section on WH-preposing), and that a condition should and can be placed on the grammar which excludes categories being generated by base rules which are "filled" only by grammatical transformations and never by lexical insertion.

Let us first, however, briefly consider the rules that Hooper cites as violating such a condition. The agent-postposing part of the passive rule, in a grammar where the agentive by is sub-categorized as a strictly intransitive preposition in the base (+___), is not really a violation if it is formulated as an obligatory rule, since Emonds (1970) never considers that empty nodes can count as violating such a sub-categorization feature. On the other hand, there is no reason in the structure-preserving framework why agentive by must appear in the base; it could also simply be inserted by the agent-postposing rule as a preposition -- recall that insertions are subject to the constraint.

Concerning indefinite subject movement ("there-insertion"), Hooper gives an alternative analysis which is structure-preserving and which does not violate the desired condition prohibiting nodes which are always empty in deep structure. On the other hand, I do not agree that she has shown that none of the arguments in Emonds, 1970 in favor of a particular structure-preserving formulation of indefinite subject movement hold up, given a less radical reformulation than hers. This will be gone into in detail in Emonds (to appear) in the appropriate section. The important point here is that the noun phrase following a be with expletive subject there has properties of a predicate nominal noun phrase, which can be explained by formulating "there-insertion" as a (structure-preserving) replacement of an empty node, thereby providing a positive argument for the structure-preserving constraint. (If, as argued in Jenkins, 1973 there is no movement rule involved, obviously the whole discussion is irrelevant.)

I should add that in the reformulation I favor, I abandon the idea that empty nodes can fulfill an obligatory sub-categorization restriction. That is, I now take the position that empty nodes count as nothing at all in determining whether sub-categorization restrictions and requirements are fulfilled.

In order to answer Hooper's criticism of my formulation of Spanish and French Clitic Placement, I must reformulate the base rules for clitics in these languages as either V----CL – V or V----V – CL, with recursion permitted. According to Kayne (1973) and Ruwet (1972b), CL is filled in deep structure in French by obligatorily reflexive verbs and also by certain verbs which must appear with the intrinsic clitic en. (The independence of such an en from a de-phrase is shown by lexical entries such as en avoir marre de NP, where both appear.) In fact, some lexical items are subcategorized to appear with both a reflexive clitic and en: s'en prendre à NP, s'en aller, etc. There are also obligatorily reflexive verbs of the same type in Spanish.

Such a revision entails giving up the idea of capturing generalizations on the internal ordering among clitics by means of phrase structure rules, as in Emonds, 1970. Thus, one must account for this ordering either by transformations or extend grammatical theory to include the positive "surface constraints" of Perlmutter (1969). (As noted by Ruwet (1972), such "constraints" increase rather than decrease the class of possible grammars.) In the case of French, at least, it seems to me that a full account of *all* restrictions on French clitics, including explicit rules for assigning strong pronouns different forms than clitics, favors a transformational account. But in any case, accepting positive surface constraints within the structure-preserving framework cannot be objected to be one who accepts them in a less constrained theory.

Finally, Hooper's criticism applies to the analysis of WH-preposing in Emonds (1970), an analysis that has been substantially revised in Emonds (to appear) so as to no longer be subject to it. In my earlier analysis, I permitted NP, PP, and AP to be optionally generated in the sentence-initial COMP ("complementizer") position -- thereby making WH-fronting structure-preserving.[3] I claimed that one could insure that these nodes were never filled with lexical material in the base because "no semantic projection rule or rule of interpretation operating on deep structure refers to them." However, if one were to permit free generation of base nodes that are always empty on these grounds, any formal claim about possible languages in terms of the structure-preserving constraint would be greatly weakened and perhaps nullified, as Hooper has noted.

Thus, in the present study, I exclude the possibility that semantic projection rules defined on deep structure -- if they exist -- can play a role in "filtering out" whole sets of base structures that have anything but an empty or dummy node in a given base position. That is, for every position of a node C on the right side of a base rule expanding a node B, there is a semantic projection rule operating on deep structures which takes "B over C," with C in this position, as part of its input.[4]

This restriction excludes a base rule that generates some node C in a position where C never appears filled in any well-formed derivation in its deep structure position.

It should be noted that the above restriction on semantic projection rules simply specifies that *if they exist*, they cannot play a central "generative" role of "filtering out" configurations otherwise permitted by the syntax.[5] If they do not exist, *nothing need be said* -- any node generable in deep structure will be filled by general processes of lexical insertion (subject to possible contextual restrictions on the inserted and surrounding lexical items). These clarifications were pointed out to me by M. Ronat.

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[3] As pointed out in Emonds, 1970, there are some striking restrictions on WH-fronting indicating that it is a structure-preserving rule. The only alternative analysis of WH-frontint which accounts for these restrictions in terms of general conditions on applicability of transformations (without resorting to *ad hoc* conditions) is the one given in Emonds (to appear).

[4] It may be possible to impose the stronger restriction that every configuration generable by a base rule must be the input to some semantic projection rule. However, I do not rule out at this point a possibility like a base rule of the form: $A \dashrightarrow (B) - \left\{\begin{matrix} C \\ D \end{matrix}\right\} - (E)$, all of whose expansions are semantically interpretable save, say, B-C-E.

2. *Possible Generalizations of the Constraint.*

At first glance, when one compares the above formulation of the structure-preserving constraint to some rather obvious grammatical processes in languages other than English, it appears that the constraint is seriously inadequate. However, it seems likely to me that the constraint can be generalized in certain ways so that the general idea of the constraint remains intact. I will discuss here two types of such inadequacies, and suggest ways that the structure-preserving constraint might be revised in order to account for these cases.

The most general form of the structure-preserving constraint may contain language-specific variables other than the set of phrase structure or base rules, so that the above statement of the constraint would be in fact a special case of a more general formulation.

In particular, in some languages where movement transformations are more freely applicable in certain embedded clauses, a somewhat larger class of nodes may play the role that root S's play in English and French. (This possibility has been suggested by Ken Hale.)

As a case in point, the analysis of Classical Arabic given by Saib (1972) suggests a generalization of the structure-preserving constraint in this direction. Saib argues convincingly that the verb is first in deep structure in Classical Arabic, and that the two types of subject-verb-object order which do occur in surface structure (besides the verb-subject-object order) are transformationally derived. These orders are:

$$\text{(a)}\ \begin{bmatrix}\text{NP}\\ \text{+NOM}\end{bmatrix} - \text{V} - \begin{bmatrix}\text{NP}\\ \text{+ACC}\end{bmatrix} \qquad \text{(b)}\ \begin{bmatrix}\left\{\begin{matrix}\text{'inna}\\ \text{'anna}\end{matrix}\right\}\\ \text{COMP}\end{bmatrix}\begin{bmatrix}\text{NP}\\ \text{+ACC}\end{bmatrix} - \text{V} - \begin{bmatrix}\text{NP}\\ \text{+ACC}\end{bmatrix}$$

Order (a), according to Saib, occurs only in root S's, in accordance with the structure-preserving constraint. Order (b) is an optional variant or the verb-subject-object order in main clauses and in certain *limited* types of subordinate clauses. The deep structure verb-first order is *always* possible, although the clause in question is sometimes introduced an *'an* and sometimes by *'annahu*.

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[5] One can probably make the same claim about phonological rules, namely, that various surface configurations cannot be ruled out only because no phonological rules apply to make them "pronounceable." For example, one would not want to say that English is lacking post-positions because phonological rules apply only to P-NP and not to NP-P. In other words, what is said here and in the text is a reiteration of the claim in Chomsky, 1965 that semantic and phonological rules are in some sense "non-generative" or "purely interpretive." One does not, of course, exclude the possibility that some phonological *lexical* gaps exist when one claims that phonological rules can interpret any well-formed syntactic input, which includes lexical items.

As Saib has pointed out to me, order (b) is a counterexample to the structure-preserving constraint if the definition of root S given here is to hold for Arabic. (Since the rule which moves an NP to the left of the verb applies only in subordinate clauses introduced by 'annahu, and since it sometimes moves the object NP, which is not adjacent to the verb, it cannot be a minor movement transformation.) Thus, it may be that in Classical Arabic certain complement S's -- those introduced by 'anna(hu) -- can play the role of root S's, in line with the suggestion of Hale's mentioned above.[6]

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[6] The types of subordinates clauses that order (b) occurs in are similar, I believe, to the indirect discourse contexts in which the structure-preserving constraint is sporadically violated in English, giving sentences of doubtful acceptability. In Arabic, however, the acceptability of the (b) order in the appropriate contexts is not in doubt, as in English.

In Hooper and Thompson, 1973, it is argued that certain dependent clauses even in English (those which are "semantically asserted," in their terms) play the role of root S's, suggesting that the definition of root S as given above may be too restrictive even for English. At present, Hooper and Thompson's notion of assertion can be formalized as a list of different clause types, such as some feature +ASSERTION to be associated with main clauses and certain other structurally definable clauses.

I cannot yet evaluate these claims, as I have not yet been able to systematically investigate the data in areas where different claims than mine are made. My impression at the moment is that their alternative analysis faces as many counterexamples as mine (not the same ones, by any means, which gives the alternative proposal interest), and that there is also, unfortunately, much disagreement over the acceptability of the data in crucial areas. In some cases, however, I am not sure that really different claims are made. For example, it may be that non-restrictive relative clauses, which Hooper and Thompson consider to be semantically asserted, are derived from a coordination of root S's by means of the root transformation of parenthetical formation discussed in Emonds, 1973:

John came in late, and he is usually reliable ⇒
(by Parenthetical Formation, which here postposes the first VP)
John, and he is usually reliable, came in late ⇒
John, who is usually reliable, came in late.

Such an analysis correctly predicts that the following example is unacceptable, because parenthetical formation normally moves only constituents:

?Bill asked John, who is usually responsible, for the rent money last Saturday.

Part of the evidence that should be used in determining whether such a derivation of non-restrictive relatives is correct is of course whether or not root transformations freely apply in such clauses, as Hooper and Thompson claim. But if they do, it seems that the above analysis makes unnecessary a special extension of the notion "root S of English" for non-restrictive relatives.

Similarly, Hooper and Thompson claim that "non-restrictive" subordinate clauses such as (i) freely allow root transformations:

(i) John isn't coming, because I just talked to his brother.
≠ (ii) John isn't coming because I just talked to his brother.

However, Edward Klima (personal communication) has pointed out to me that preposability is a characteristic of subordinate, but not coordinate clauses:

However, at this point I am not in a position to hypothesize what are the possible sets of nodes that may function as root nodes in languages other than those like English. A reasonable working hypothesis would be that only S nodes can play the role of root nodes in any language; a stronger one would be that only left- or right-branching S nodes can play this role. Interesting work in this regard I must leave for future research.[7]

A second kind of violation of the structure-preserving constraint appears to occur when certain phrasal constituents move about within a sentence somewhat freely. For example, Banfield (1973b) notes that certain "stylistic transformations" which account for the "free phrase order" in English poetry of the grand style (Milton, Donne, Pope, etc.) are neither root nor structure-preserving operations. It is to be noted, however, that such stylistic transformations exhibit, according to Banfield, certain other formal characteristics which differentiate them from strictly grammatical transformations: they follow all other syntactic rules including agreement and case assignment; they cannot be "triggered" by the presence or absence of specific morphemes in the tree (by contrast, say, to English dative movement which is "triggered" by the prepositions to and for); they cannot introduce morphemes, nor delete morphemes except under identity; they appear to be subject to some version of the "up-to-ambiguity" principle of Chomsky (1965:126-7); and informally, their inputs appear to be systematically as acceptable or more acceptable (even in the case of deletion under identity) than their outputs.[8] For discussion of these characteristics, see Banfield (1973b, chapter 3).

(footnote 6 continued)

John isn't coming, $\left\{\begin{array}{c}\text{but}\\ \text{although}\end{array}\right\}$ the rest of the family will attend.

$\left\{\begin{array}{c}\text{Although}\\ \text{*But}\end{array}\right\}$ the rest of the family will attend, John isn't coming.

John isn't coming, $\left\{\begin{array}{c}\text{for}\\ \text{because}\end{array}\right\}$ the car isn't running.

$\left\{\begin{array}{c}\text{Because}\\ \text{*For}\end{array}\right\}$ the car isn't running, John isn't coming.

By this test, the non-restrictive use of because in (i) is a coordinating conjunction, and hence it introduces a root $\bar{S}$:

(iii) Because I just talked to his brother, John isn't coming.

(iii) means the same as (ii), but not the same as (i). Thus, I agree with Hooper and Thompson that such because-clauses are root S's.

There is of course no requirement in the structure-preserving constraint that all root transformations are applicable in all root S's.

[7] If in fact some languages have a larger set of root nodes than does English, another question that is immediately raised is whether such variation is random or related to other formal properties of the languages in question.

[8] As Banfield points out, this is not the case with some optional grammatical transformations: "John washed the big rug and Bill washed the big rug too." In many ways these stylistic transformations resemble the feature-changing performance rule proposed by Otero (1972) for certain acceptable but ungrammatical utterances of Spanish -- in particular, the "up-to-ambiguity" characteristic and the impression that they are partly conscious "intervention for effect" by the speaker.

A revision of the structure-preserving constraint of the following form therefore suggests itself: Under certain conditions, stylistic (as opposed to grammatical) transformations may break the structure-preserving constraint. Clearly, it is a revision of this type that should account for many of the phenomena traditionally termed "free word order."

It is, however, equally clear that not any stylistic rule can break the structure-preserving constraint. As Banfield points out, the stylistic movement rules of Modern spoken English are root and structure-preserving rules. So it is only *some* stylistic transformations which break the structure-preserving constraint — e.g. those of English poetry in the grand style.

Another case of a violation of this type may be the movement of adverb phrases in French, which Schlyter (1973) analyzes as being subject to the "transportability convention" of Keyser (1968).[9] Jan Koster, in work in progress, makes a similar claim about adverb phrases in Dutch — i.e. that they are not subject to the structure-preserving constraint. However, the transportability convention of Keyser and the transformations proposed by Koster for Dutch adverbials do have all the characteristics of stylistic transformations given above. Similarly, Carlos Otero (personal communication) has pointed out to me the existence of a stylistic movement of subject NP's to the end of the clause in Spanish, which is not structure-preserving.

It seems, then, that the following problems present themselves: (i) under what conditions may embedded S's play the role of root S's? (ii) under what conditions (on the rule, or on the grammar in which the rule appears) can a stylistic transformation break the structure-preserving constraint? I claim that the conditions for either possibility, whatever their nature, do not exist in Modern English; i.e. that all major transformational operations in English *are* root or structure-preserving, with root S as defined earlier. But I am not claiming that careful analyses of other languages, such as those undertaken by Saib, Schlyter, and Koster, will not lead to different, though ultimately compatible, conclusions about the grammars of other languages, and to important modifications in the structure-preserving constraint. There have of course been studies of other languages confirming claims in Emonds, 1970: Quicoli (1972) for Portuguese, Koster (1973) for Dutch, and Ruwet (1973, chapter 1) for French.

3. Restrictions on Base Rules.

In the model of a transformational grammar described in Chomsky, 1965, the deep structures are generated by a set of context-free phrase structure rules and the lexicon. As research has developed and refined this model, it has become clear that the notion of "possible set of base rules"

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[9] According to Keyser, a node is "transportable" if it can freely appear before or after any of its sister constituents. He claims that English adverbial phrases are transportable, but I argue in Emonds, 1970 that this is not true.

(excluding any rules that may be in the lexicon) can be considerably restricted without loss of generality. In particular, the work of Chomsky (1970), Jackendoff (1971), Dougherty (1970), Ruwet (1969), Shopen (1972), Selkirk (1970 and 1972), Bowers (1969), and others suggest that all phrasal categories of the grammar are best defined in terms of at most four "lexical category nodes": N, V, A, and P (noun, verb, adjective, and preposition).[10] The restrictions on base rules which will be proposed here follow from this basic premise (which is also, in my opinion, implicit in the work of Z. Harris).

The adverbs which are formed from adjectives -- by adding ly in English, for example -- will be treated simply as adjectives in a verb-modifying rather than in a noun-modifying function. The basic justification for this is that such adverbs with few exceptions are formed regularly from adjectives, and that they are modified by exactly the same type of grammatical constructions (comparatives, superlatives, etc.) as are adjectives. For English and French, at least, a similar statement would not hold of any two categories of the N, V, A, P group.

As a basis for the discussion, it is useful to have a formal definition of the term "phrasal category." In order to be as non-committal as possible on non-essential points, I will not exclude the possibility that phrase nodes directly dominate certain formatives (i.e. *I* could conceivably be an NP but not an N), nor the possibility that non-phrase nodes like N may occur on the left side of certain base rules (i.e. a rule of the form N--→ V – P could be the source of compound nouns like *lean-to*, *walk-on*, *take-off*, *build-up*, *talking-to*, *run-about*, etc. as suggested in Kuiper, 1972). So I will not simply identify phrase nodes with nodes that don't dominate lexical entries, nor with being the only nodes that occur on the left of (non-lexical) base rules, although one or both of these may in fact also be properties of phrase nodes.

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[10] One might object that a "non-productive" category P should not have the same status as the "productive" categories of N, V, and A, which have several hundred or even thousand members, and for which new members can be "coined" by speakers of the language. But there is nothing in the restrictions on base rules to be proposed here that conflicts with these differences in properties. Concerning the claim that lexical categories must be "productive," a relevant observation has been made by Vida Samiian, an Iranian linguistics student, who feels that the standard literary dialect of Modern Persian would be best analyzed as having only noun and adjective as productive categories, verbs being a non-productive category.

"Preposition" as a name for the fourth lexical category is an unfortunate inheritance from traditional grammar. On the one hand, the category includes many "intransitive prepositions," which are not "preposed" in any sense, such as together, away, back (cf. Emonds, 1972); on the other, many elements traditionally termed prepositions, such as à and de in French, may well not be in that lexical category (cf. Ruwet, 1969). A better term which would also have grammatical tradition behind it might be adverb; however, many elements traditionally termed adverbs (e.g. adjective-modifying words like English very) are not members of this category. Further, the category in question can modify nouns as well as verbs ("the weather around the airport"), and adverb cannot be written with a single mnemonic symbol, because A = adjective. Thus, I will continue the usage that has begun in transformational grammar, and retain the symbol P for the fourth lexical category.

A definition of phrasal category compatible with a model in which neither, one, or both of the preceding properties hold is the following:

Phrasal Category. A grammatical category which can dominate NP by virtue of applying the base rules in a given language (without using lexical rules) is a "phrasal category" or a "phrase node" in that language. All other categories are "non-phrasal."

For example, N may dominate NP in $[_N\ [_{NP}$ American history$]_{NP}$ teacher $_N]$ and V may dominate NP in $[_V$ take $[_{NP}$ unfair advantage$]_{NP}$ of $_V]$, but these configurations can arise only if lexical insertions with internal syntactic structures are utilized.

It follows from the definition that no non-phrasal category can dominate a phrasal category. (If it could, it could dominate NP, and hence would itself be phrasal.) The nodes which one might informally call "phrase nodes" in grammatical descriptions of English (S, NP, VP, AP, PP) clearly satisfy the above definition. (Note that the definition does not specify immediate dominance.) On the other hand, descriptions of English have been proposed in which DET --→ NP is a phrase structure rule (for generating possessive noun phrase), which would mean DET was a phrasal node. We will see below that such a phrase structure rule is excluded by the restrictions we will place on base rules, which would dictate rather an alternative such as NP --→ $\begin{Bmatrix}\text{DET}\\ \text{NP}\end{Bmatrix}$ ... N ... for possessive noun phrase; in the appendix of Emonds, 1970, corroborative empirical evidence is given that in this case at least the formal restrictions to be proposed here have the right result.

It seems that base rules are permitted by which any phrasal category can dominate any other. In the case of English, the base rules proposed here will yield almost every possible combination of *immediate* dominance among the phrase nodes S, NP, AP, VP, and PP. (A non-occurring case might be PP --→ ... VP ...; PP apparently immediately dominates even AP in sentences like "The atmosphere changed from very depressing to quite gay when she arrived.")

Given this fundamental distinction between phrasal and non-phrasal categories defined by the base rules, we can proceed to formalize a number of restrictions on base rules that have generally been observed in practice by transformationalists, if not explicitly recognized.

Definition. The four categories N (noun), V (verb), A (adjective), and P (preposition) are "lexical categories."

Lexical Restriction. Only lexical categories can be "productive," in the sense of note 6.

Base Restriction I. Each lexical category of a language L appears exactly once on the right side of one base rule of L.[11]

One might be tempted to extend Base Restriction I to all non-phrasal categories. However, there are plausible analyses of negation, questions, etc. which propose more than one deep structure source for morphemes like WH and NEG, so any simplistic extension of this type appears premature at best. On the other hand, one might be able to impose the requirement that any non-phrasal category can appear but once in the base rules, except that it may co-occur as an optional syntactic feature on some other category without breaking the restriction. Thus, a feature like NEG could have a "principal" position in the base — say in the auxiliary in English — and could also appear as an optional feature on other categories, such as DET and/or COMP. (An analysis of negation compatible with such a restriction would be that of Lasnik, 1972). But I will not pursue this further here.

The specification of possible base rules using Base Restriction I which has been developed in the works cited above (Chomsky, Jackendoff, Dougherty, Ruwet, etc.) depends crucially on utilizing the notion "head of a phrase." This signals an attempt at formulating within a generative model a central principle in the work of Z. Harris. Further refinements and modifications in this area will no doubt depend crucially on how exactly the generative model can capture some of the properties of heads of phrases defined and discussed in, for example, Harris, 1957 and Harris, 1958.

In defining further restrictions here, I will use neither the superscript notation of Harris nor the "bar notation" of Chomsky, but rather the "prime" notation of Selkirk (1972). This has typographical advantages, and is also meant to reflect the fact that no formal properties of either Harris's or Chomsky's notation will be used unless they are explicitly defined here. Also, I will use the symbol H for "lexical category" or "head of a phrase," as I feel that Chomsky's "X" is too easily confused with a variable for a string.

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[11] If one considers that base rules must also be used to define the possible categorial combinations in derivational morphology (such as the N--→ V- P rule for compounds like lean-to and walk-on), then Base Restriction I applies to only those base rules with phrasal categories on the left side. The set of possible rules of this latter type is further specified below.

The only base rules for expanding phrase nodes that seem to have gained some acceptance,[12] except those for expanding S and those for generating coordinate conjunction (which will be taken up below), are of the following form:

Base Restriction II. All phrase node expansions, except those of S and of coordinate conjunction, are of the following form:

(i) H'' --→ X_1H'' X_2, where X_1X_2 does not contain H''

(ii) H'' --→ X_3H X_4

(iii) H'' --→ X_5 H' X_6, where X_5X_6 does not contain H'

(iv) H' --→ X_7 H X_8

In (i)-(iv), H is a lexical category node and X_i is a variable.[13]

Definition. In (i)-(iv), the category H, H', or H'' on the right side of the rule is called the head of the phrase H'' or H' on the left side of the rule.

It should be notted that Base Restriction I prevents any lexical category from appearing in X_i. It also insures that there will be at most the base rule of the form of (ii) or (iv) for each lexical category. In more usual terminology, which will for the most part be retained in the text, N'' = NP, V'' = VP, A'' = AP, and P'' = PP.

Since the restrictions being discussed are intended to put limits on possible base rules, it is not relevant here to precisely determine what even narrower limits can be set on base rules in an adequate grammatical theory; if more such restrictions can be imposed without loss of explanatory adequacy elsewhere in the theory, so much the better. In particular, it is quite possible that there do not exist possible phrase nodes "between" NP and N, VP and V, etc. Thus, it may be that H' ($\overline{H}$ in the bar notation of Chomsky, 1970) does not exist separately from H'', so that only the phrase node expansions given in (i) and (ii) are theoretically possible. In this study, in fact, I do not utilize H' nodes, unless I explicitly note the contrary.

A third restriction on base rules that can be imposed is that all complement and modifier constituents of phrasal category are optional (at the deep structure level), whereas heads of phrases are obligatory. Another way to put this, which will prove useful later, is as follows:

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[12] In part, this may just reflect the fact that the languages extensively investigated in generative frameworks have been limited in number. But making precise hypotheses about base rules in general is the only way to see if and exactly how other languages require that we modify these hypotheses.

[13] It will be of interest later that these restrictions exclude NP --→ S or NP --→ NP – NP (unless the NP on the right is the head of the construction) as possible base rules. They do not, of course, prevent equivalent configurations in surface structure that might arise as a result of deletion rules.

Base Restriction III. In the meta-theoretical statements of possible base rules, the phrase nodes contained in the variables are optional; the specified phrase nodes and lexical nodes are obligatory.

As far as Base Restriction II is concerned, Base Restriction III simply means that the phrase nodes in the variables X_i are optional and that the heads of phrases are obligatory. The observation of this characteristic I would attribute to Harris (1957), although I believe that it can be accurately formulated only in a framework which admits of deep structure.

There are head nouns, verbs, adjectives, and prepositions whose lexical entries require obligatory complements of various sorts, but there are no object, adverb, predicate attribute, relative clause, or comparative complements which must appear within all H'' structures given by a particular base rule.

On the other hand, it does appear that heads of phrases are obligatory at the level of deep structure. For example, in English, the deep structure noun one is deleted after the determiner (but not after an adjective) in many contexts, yielding surface noun phrases of the form the good, the just, which, some, all, the shorter, the first, etc. Thus, "these small lamps are beautiful, but some big ones are ugly," and "these lamps are beautiful, but some are ugly." In the case of mass nouns, English lacks a lexical "pro-noun" like one, so that the string that results when the head-deletion rule does not operate is "*that dark wood is beautiful, but some light is ugly." (Cf. "that wood is beautiful, but some is ugly," where the noun-head deletion rule has applied, giving a grammatical sentence.) The fact that this string is ungrammatical indicates that the dummy terminal symbol Δ is present in the surface structure, which in turn indicates that it must have been obligatorily generated in deep structure. (It is a condition on derivations that the dummy terminal symbol Δ must be removed either by deletion or by lexical insertion — see Chomsky, 1970.) Of course, it is also well-known that surface nouns as well as verbs can be deleted ("gapped") under idenity. (Cf. Jackendoff, 1971) for a comparative study of the two cases.) In these cases also, the most convincing analyses in the end hypothesize obligatory heads of phrases in deep structure.[14]

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[14] I do not put forward any hypothesis for predicting when non-phrase nodes in the expansions of H'' and H' are optional. While most such nodes seem optional, the French article (or determiner) and TENSE in English and French are potential counterexamples to any claim that all such nodes are optional. Similarly, Van Riemsdijk (personal communication) argues that in Dutch each predicate contains an obligatory assertion or negation marker in deep structure. So I will continue to assume that nodes which are neither phrase nodes nor lexical category nodes must be marked as optional or obligatory in each base rule.

We can now approach the question of whether the subject NP node in English is dominated by V''; if it is not, then V'' cannot be identified with S. In the literature, one finds differing opinions: Chomsky (1970) keeps the two distinct, while Jackendoff (1971) does identify them.

There are two ways in which the subject NP node in a sentence S differs from other non-head phrase nodes in English: (i) it is obligatory in deep structure, independent of the selection or sub-categorization features of the verb (although it may consist only of a dummy element which is filled in during the transformational derivation with an expletive subject such as it); (ii) it is a phrase node to the left of the head node (the VP) which exhibits free right-branching recursion in surface structure. I return below to this second characteristic of the subject NP, but for the moment let us use the first in order to determine what the form of initial symbol (S) expansions should be.[15]

If the base expansions for S are to be subject to Base Restriction III as are the expansions for other phrase nodes (those given by Base Restriction II), then the obligatory subject NP and predicate VP nodes must be specified in the appropriate meta-theoretical statement (Base Restriction IV).

Base Restriction IV. The possible base rules for expanding S are of the following form:

(v) $S \dashrightarrow Y_1\ N''\ Y_2\ V''\ Y_3$, or $S \dashrightarrow Y_4\ V''\ Y_5$, where Y_i is a variable and Y_i does not contain N'' of V''.

Like the schema (i)-(iv) for expanding other phrase nodes given in Base Restriction II, (v) is subject to Base Restriction III.

It is plausible that (v) should be generalized, even for English, to include predicates which are verbless NP's, AP's, and PP's. There are, of course, languages where the copula does not appear in certain surface sentences of the form NP-NP, NP-AP, or NP-PP. But even when finite forms of the copula do appear before predicate attribute NP's, AP's, and PP's, they may be -- in some languages at least -- realizations of the node TENSE or some node other than the node V.[16] In this view, sentences like

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[15] Some recent work by Banfield (1973), Shopen (1972), and Quang Phuc Dong (1969) indicates that there is a non-recursive initial symbol E or U (Expression, or Utterance) in which non-embedded epithets, expressions and constructions occur; this symbol in turn can dominate the highest S of a sentence. The arguments for this position seem convincing to me, so that the problem being dealt with in the text is actually the determination of the expansions of the node which can dominate propositions.

[16] TENSE might better be considered as the node which marks the presence of an assertion, with (in English) +PAST as an optional accompanying syntactic feature. It should be noted that nothing in what has been said so far prevents base rules such as VP--→AUX – VP, VP--→. . . V . . . VP, or VP--→AUX– . . . V . . . VP. I take up this question in Emonds, 1973, but the restrictions set up here do not pre-determine our choice of analyses.

"John is a doctor," "John is very nice," or "John was behind the house" would contain no verb at all. If there are such verbless deep structure sentences (whatever other category name is given to the copula), then we would want to extend the schema for the base expansions of S from (v) to (vi):

(vi) $S \dashrightarrow Y_1\ N''\ Y_2\ H''\ Y_3$, or $S \dashrightarrow Y_4\ H''\ Y_5$, where Y_i is a variable, Y_i does not contain N'' or H'', and H is a lexical category node.

Let me now return to the observation made earlier that the subject NP in English, *unlike* phrase nodes to the left of the head within an NP, AP, PP, or VP, exhibits free right-branching recursion in surface structure. In contrast, such recursion is not exhibited in English by possessive noun phrase (*the king of England's* hat), pre-nominal adjective phrases, pre-adjectival measure phrases (*three tiring miles* long), intensifying adverbial adjective phrases (*so magnificently* attired, *quite simply* boring), pre-verbal adverbial adjective phrases (*politely* put it away, *very quickly* finished his work), or pre-prepositional measure phrases (put it *three feet* beyond the table). I have not investigated in detail how this restriction on recursion in surface structure can be best stated, but it certainly seems to exist. The important point is that this restriction does not apply to subject NP's, and hence this corroborates the prediction made by Base Restriction III that subject NP's are not within the VP = V''.

However, for purposes of making this claim somewhat more precise, I will venture a partial formulation of a "surface recursion restriction."

Surface Recursion Restriction. Given a surface configuration of the form:

$$[_{H_i''} \ldots H_j'' \ldots H_i \ldots H_k'' \ldots]_{H_i''}$$

then H_j'' will not exhibit free self-embedding or right-branching recursion.

Quite clearly, this restriction, if correct, excludes the possibility that V'' dominates the subject NP. It should also be noted that the restriction as stated does not apply to sentences (S). And in fact, not only subject NP's but also pre-subject adverbial phrases exhibit right-branching recursion: "The day that John said that ..., Bill died." Further, the restriction does permit, say, an NP in the surface configuration $[_{VP} \ldots NP \ldots V \ldots S]$ to have surface recursion, because only H'' (and not S) to the right of the head prevents recursion before the head. It has been pointed out to me by H. Van Riemsdijk that this possibility should be allowed for the analysis of Dutch and German.

A consequence of the surface recursion restriction is that either all right-branching recursive phrase nodes (except perhaps S) appear to the left of the head of a phrase in surface structure, or else all appear to the right of the head. Thus, it makes sense to speak of the "recursive side of (the head of) a phrase" as that side of the head which exhibits all the (right-branching) recursion.

Finally, we must specify the possible base rules for coordinate conjunction of phrase nodes. In this, I use the rules of Dougherty (1970). He has demonstrated, in my opinion, that all coordinate conjunction in English, except what may be termed conjunction across constituent boundaries,[17] should be generated by the base; he proposes the following base rules:

> $X \dashrightarrow (Q)\ X^n\ (ADV)$, where X = S, NP, or VP; Q (the "distributive quantifier") = *each*, *all*, *both*, *either*, *neither*; ADV (the "distributive adverb") = *at once*, *simultaneously*, *concurrently*, *en masse*, *in different directions*, *respectively*, *correspondingly*, *at one time*, *together*, etc.

I will replace Dougherty's X by H'' and S to be consistent with what has gone before here, and slightly generalize his formula for base conjunction in writing a schema for possible base rules for conjunction. Conceivably, X should be extended to H and/or H' as well.

Base Restriction V. The possible base rules for coordinate conjunction of phrase node are of the following form:

(vii) $H'' \dashrightarrow U_1\ H''^n\ U_2$; $S \dashrightarrow U_3\ S^n\ U_4$; U_i is a variable and H is a lexical category node.

These expansions are subject to the same restrictions as are those of (i) - (vi): H'' and S are the specified nodes and hence obligatory; any phrasal categories in U_i are optional: U_1U_2 and U_3U_4 can't contain any lexical category nodes.

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[17] An analysis of coordination across constituent boundaries in terms of a stylistic deletion transformation is given in Banfield, 1973b.

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LINGUISTIC VARIABLES AND LINGUISTIC CHANGE IN OHG TEXTS OF THE NINTH CENTURY

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1. Introduction

In 1968, Weinreich, Labov, and Herzog[1] proposed a theory of linguistic change based on the facts of orderly heterogeneity within the speech community. Labov's work[2] has demonstrated the feasibility of studying linguistic change in progress on the basis of the relationship between linguistic and social variables. Despite statements to the contrary,[3] it is apparent that the study of actual linguistic changes does require "enormous" amounts of data. For linguistic changes that took place in the more distant past, it will be increasingly difficult to assemble the data required by the theory of sociolinguistic change, even in a language with an extensive written tradition. When I recently surveyed a variety of texts for a historical phonology of German, it appeared to me that one of the major Old High German documents, the so-called Tatian,[4] might be of some interest in this respect.

Around 830, a group of monks at Fulda, Germany, translated a Latin version of Tatian's Diatessaron (Gospel Harmony) into German. The MS with the Latin and German versions in parallel columns was then copied by another group of monks. Five of these, known as scribes α β γ δ ε, wrote various parts of the MS, while the sixth, known as scribe ζ, not only wrote his part but also corrected the work of his colleagues. He seems to have considered it his duty to correct not only spelling errors but also deviations from the standard dialect of Fulda at that time. As the following figures show, scribe γ seems to have deviated most from this standard.

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[1] U. Weinreich, W. Labov, and M.I. Herzog, Empirical Foundations for a Theory of Language Change, in W.P. Lehmann and Y. Malkiel (eds.), Directions for Historical Linguistics, University of Texas Press, 1968, 95-195.

[2] W. Labov, The Social Motivation of a Sound Change, Word 19, 1963, 273-309.

[3] "Bloomfield, for example (1933:365ff), thought that nondistinctive changes are observable only by the phonetician who has at his disposal 'an enormous mass of mechanical records, reaching through several generations of speakers' ... Non-distinctive variation has, in fact, been observed by Labov (1963, 1966) through far from 'enormous' samples... Lehmann, for example (1962: 148), does not hold that subphonemic changes are unobservable, only that they are not taken into account by scribes rendering their language in phonemic terms. It is for this far-better-formulated reason that linguists have little information about nondistinctive changes of the past." (Weinreich, Labov, and Herzog, 1968: 130 and fn. 32)

[4] E. Sievers (ed.), Tatian. Lateinisch und altdeutsch mit ausführlichem Glossar, 2nd rev. ed., Paderborn, 1892.

B. Mohr, The phonology of the OHG Tatian Translation, unpubl. MS, 1970.

(1) Average number of corrections per page

α	β	γ	δ	ε
1.2	2.5	9.2	1.9	2.6

A close study of the speech (and writing) habits of the several scribes reveals distinct individual characteristics. These can be placed into the framework of the historical development of the Fulda dialect, illustrated by the yearly succession of Monastery Records from ca. 750 to 850 (MON.REC.)[5] and a number of other documents associated with Fulda (HILD. = Hildebrandslied, ca. 780; SAL.LAW. = Bruchstück der Lex Salica, ca. 800; HAM.B.D. = Hammelburger Markbeschreibung, ca. 820; FULD.CONF. = Fuldaer Beichte, ca. 830).[6]

2. Phonological data

2.1. The so-called (High) German consonant shift changed voiceless stops into affricates. These affricates were then simplified to fricatives under certain conditions. The extent of these changes in the documents under discussion is shown below.

(2) Affrication and spirantization of *p

	#__			V__			R__		
*p	p	pf	f	p	pf	f (f)	p	pf	f (f)
HILD.	(no examples)			2	-	-	2	-	-
MON.REC.	(no examples)			1	2	7 (70%)	1	1	6 (75%)
Tα	-	3	-	-	-	X (100%)	-	12	-
Tβ	-	9	-				-	38	2 (5%)
Tγ	-	4	4 (50%)				-	4	20 (83%)
Tδ	-	6	1 (14 %)				-	1	12 (92%)
Tε	(no examples)						-	2	-
Tζ	-	7	-				-	27	6 (18%)

HILD. shows only unaffricated stops. MON. REC. still shows a few unaffricated stops, some affricates, but already 70-75% fricatives in medial position. TATIAN shows no unaffricated stops in any position, and no affricates after vowels. After resonants, however, Tα and Tε have no fricatives at all, Tβ and Tζ have a few fricatives, but Tγ and Tδ have almost always fricatives. Tγ and Tδ even have a small number of fricatives in initial position.

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[5] E.F.J. Dronke (ed.), Codex Diplomaticus Fuldensis, Cassel, 1850; repr. 1962.

G. Kossinna, Über die ältesten hochfränkischen Sprachdenkmäler (= Quellen und Forschungen zur Sprach- und Kulturgeschichte der Germanischen Völker, 46), Strassburg, 1881.

[6] E. von Steinmeyer (ed.), Die kleineren althochdeutschen Sprachdenkmäler, Berlin, 1916, 1-15; 55-59; 62-63; 327-329.

W.G. Moulton, Scribe γ of the Old High German Tatian Translation, PMLA 59, 1944, 307-334.

The dental stop is affricated in initial position and after resonants, spirantized after vowels only. The velar stop remains unaffricated but is spirantized after vowels. All documents show this distribution; only HILD. has a few unchanged stops which may have been inserted when this originally High German piece was adapted for a Low German audience.

2.2 The consonant shift also changed voiced stops into voiceless stops, but certain aspects of this change are confined to the Uppermost German dialects. The extent of these changes in the documents under discussion is shown below.

(3) Devoicing of *b

		#_		X_	
*b		b	p	b	p
MON.REC.	750-764	24	57 (70%)	8	21 (72%)
	765-776	68	23 (25%)	96	5 (5%)
	777-800	193	33 (15%)		
	800-813	114	57 (33%)		
	813-841	135	41 (23%)		
HILD.		32	2 (6%)	(no examples)	
SAL. LAW.		5	–	19	2 (10%)
HAM. B. D.		22	1 (4%)	(no examples)	
FULD. CONF.		9	–	(no examples)	
Tγ		218	4 (2%)	X	–
Tαβδεζ		X	–	X	–

MON. REC. shows that this sound change was unsuccessful at Fulda, showing a steady decline from an initial high of 70-72% to a later low of 5% in medial position. All other Fulda documents show a very low incidence of the voiceless stop, and among the Tatian scribes it is only Tγ who shows a few (2%) voiceless stops as a mark of the older tradition.

The devoicing of the velar stop was even less successful: MON. REC. shows only 4 voiceless stops among 600 voiced stops. No other document shows the voiceless stop.

2.3 The development of the voiced dental stop is quite different. It has the following distribution in the Fulda documents.

(4) Devoicing of *d

	#__		X__	
*d	d	t	d	t
MON. REC. 750-774	3	17 (85%)	37	83 (69%)
775-789			61	89 (59%)
800-812			26	76 (75%)
812-841			16	115 (88%)
HILD.	-	98 (100%)	-	X (100%)
SAL. LAW.	1	78 (98%)	-	X (100%)
HAM. B. D.	-	X (100%)	-	X (100%)
FULD. CONF.	-	X (100%)	-	X (100%)
Tα	9	1048 (89)	-	X (100%)
Tβ	15		-	
Tγ	12		-	
Tδ	14		7	
Tε	4		-	
Tζ	75		1	

In medial position, MON. REC. shows a decline in the occurrence of the voiceless stop (as expected on the basis of the discussion in the preceding section) up to about 800. After 800, there is a rapid increase, approaching 90% by 850. In initial position, the voiceless stop occurs 85% in MON. REC., 89% in TATIAN. All other documents show the voiceless stop in virtually all cases.

Even though all of the Tatian scribes show some occurrences of the older voiced stop in initial position, its occurrence is restricted to a small number of words as shown below.

(5) Number of words with initial d or t in Tatian.

	No. of words with some d	No. of words with t only
TATIAN	15	18 (55%)
Tα	3	30 (91%)
Tβ	4	29 (88%)
Tγ	4	29 (88%)
Tδ	7	26 (79%)
Tε	1	32 (97%)
Tζ	10	23 (70%)

It is apparent that scribes Tζ and Tδ represent a less advanced stage, while scribes Tα and Tε represent a more advanced stage. These figures show that phonological change spreads gradually not only through the speech community (transmission) but also through the lexicon (diffusion).[7] Lexical diffusion in this case can be shown even more clearly in the following tabulation.

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[7] Typical sound change trajectories are shown in an Appendix at the end of this paper. These trajectories are graphical representations of the gradual progression of sound changes through time, through the community, and through the lexicon.

(6) Frequency of occurrence of initial t < d in selected Tatian words

Selected words		Frequency of t
diuri	'dear'	22
duri	'door'	37
duoh	'cloth'	40
durran	'dare'	50
duala	'stupor'	50
dohter	'daughter'	54
duom	'judge'	64
dumb	'dumb'	70 (%)
dragan	'carry'	83
dougali	'secret'	87
deil	'part'	92
dōd	'death'	93
duon	'do'	93
dag	'day'	97
drinkan	'drink'	98
tal . . . truoben (18 words in all)	'valley' 'mourn'	100%

2.4. The data presented in 2.2 and 2.3 above raise the question of why the change of the voiced dental stop was successful while that of the labial and velar apparently failed. It is usually suggested that there must have been a connection between the change of *d > t and the change of *þ [θ] > d. The distribution of the reflexes of the interdental fricative *þ in the documents under discussion is shown below.

(7) The development of *þ

*þ		#_ th	đ	d		X_ th	đ	d
MON. REC.	750-774	20	-	7 (26%)		-	-	X (100 %)
	775-796	16	-	1 (6%)				
	797-804	27	-	8 (23%)				
	804-841	39	-	2 (5%)				
HILD.		1	2	58 (95%)		-	2	27 (93%)
SAL. LAW.		-	28	-		-	21	1 (5%)
HAM. B. D.		12	-	-		-	-	11 (100%)
FULD. CONF.		16	-	-		1	-	17 (95%)
Tα		5820	6	3	233 (4%)	-	1	X (100%)
Tβ			-	4		-	-	
Tγ			-	155		1	-	
Tδ			-	5		-	-	
Tϵ			-	-		-	-	
Tζ			-	66		4	-	

The data seem to illustrate two partially conflicting, partially overlapping tendencies. One of the changes replaced *þ by đ (SAL. LAW.) or d (HILD.) in all positions. Among the Tatian scribes it is only Tα who shows even a trace of this change. MON. REC. shows that the change was unsuccessful in initial position where the occurrence of d declines from 26% to 5%.

The second change replaced *þ by d in medial position (the unchanged spirant in initial position is represented by th). This change was highly successful in all Fulda documents, including MON. REC. and TATIAN. It is beginning to spread to initial position in second members of certain fixed expressions such as the compound relatives in -thar and certain adverbial phrases consisting of a preposition + thiu (for example, bi-thiu 'because'), as illustrated below.

(8) The development of *þ in compound expressions

	-thar			-thiu	
*þ	th	đ	d	th	d
Tα	33	1	4 (11%)	41	-
Tβ	91	-	-	80	1 (1%)
Tγ	4	-	62 (94%)	14	19 (58%)
Tδ	1	-	60 (98%)	13	22 (63%)
Tε	1	-	-	8	-
Tζ	7	-	77 (92%)	63	49 (44%)

The voiced stop also occurs in initial position but restricted to certain words only.

(9) Number of words with initial th or d in Tatian

	No. of words with th only	No. of words with some d
TATIAN	22	17 (44%)
Tα	37	2 (5%)
Tβ	35	4 (10%)
Tγ	27	12 (31%)
Tδ	35	4 (10%)
Tε	39	-
Tζ	32	7 (18%)

It is apparent that in both cases (8) and (9) scribes Tαδζ represent a more advanced stage than scribes Tαβε. The following tabulation shows the degree of lexical diffusion in this case.

(10) Frequency of occurrence of initial d < þ in selected Tatian words

Selected words		Frequency of d
thara	'to there'	0 %
... thô	'then'	
(22 words)		
thanne	'then'	2 %
these(r)	'this'	2
thana(na)	'from there'	3
thaz	'that'	3
ther	'the'	4
thâr	'there'	7
thrâto	'very'	7
thû	'you'	7
thorrên	'dry'	8
thurfan	'need'	8
thing	'thing'	9
thîhan	'thrive'	14
thrî	'three'	20
thuncôn	'dip'	25
thruoên	'suffer'	33
gi-thult	'patience'	50
bi-therbi	'useful'	60

I want to point out that the data presented in 2.3 and 2.4 do not seem to support a theory which correlates the changes d > t and þ > d either as a drag chain (scribes Tα and Tϵ have the highest number of new t and the lowest number of new d) or as a push chain (scribes Tγδζ have the highest number of new d and the highest number of old d = the lowest number of new t).

2.5. Among vowel changes, the only noteworthy developments illustrated in the documents under discussion involve the diphthongization of ê, ô to ie, uo, as shown below.

(11) Diphthongization of mid vowels

		ô	ou	ê	ea	ie
MON. REC.	750-770	11	3 (22%)	-	-	1
	771-779	15	26 (63%)			
	779-790	7	19 (73%)			
	790-800	7	24 (77%)			
	800-841	-	X (100%)			
HILD.		5	6 (55%)	7	1	1 (22%)
SAL. LAW.		1	-	1	-	1 (50%)
HAM. B. D.		-	3 (100%)	-	-	3 (100%)
FULD. CONF.		-	6 (100%)	-	-	5 (100%)
Tγ		1	X (100%)	44	3	76 (64%)
Tδ		5		-	-	X (100%)
Tαβϵζ		-				

The diphthongization of ô > uo is virtually complete by 800, while the diphthongization of ê > ie seems to have lagged somewhat behind. Even so, scribe Tγ seems to be definitely old-fashioned in showing only 64% diphthongization.

2.6 The suffix *an occurred both in the Infinitive and the Past Participle of verbs. It is reduced to -en under various conditions, as illustrated below.

(12) Reduction of suffixes in *an

	Infinitive of "weak" VB in *jan		Infinitive of "strong" VB in *an		Past Participle in *an	
	-an	-en	-an	-en	-an	-en
HILD.	-	4 (100%)	9	-	(no examples)	
SAL. LAW.	-	2 (100%)	1	-		
FULD.CONF.	2	2 (50%)	4	6 (60%)		
TATIAN.	25	56 (69%)	180	32 (15%)	336	12 (3%)
Tα	4	6 (60%)	16	9 (36%)	77	1 (1%)
Tβ	1	24 (96%)	59	7 (11%)	86	4 (4%)
Tγ	3	13 (81%)	13	16 (55%)	21	1 (5%)
Tδ	11	-	31	-	46	5 (10%)
Tε	2	1 (33%)	-	-	2	-
Tζ	4	12 (75%)	61	-	104	1 (1%)

The older Fulda documents like HILD. and SAL. LAW. show the regular phonetic development of the Infinitive suffix *jan to -en in the so-called weak verbs where *j represents the derivational suffix. FULD.CONF. shows the reduction spreading to the so-called strong verbs. But since *j itself is generally deleted, -an occurs also in weak verbs. The various scribes of the Tatian MS give evidence of a similar confusion, even though the weak verbs show a higher percentage of -en forms (69%) than the strong verbs (15%). The change seems to be spreading now also to the Past Participle suffix. Even scribes Tδ and Tζ reduce this ending on some occasions although they do not reduce the Infinitive suffix of the strong verbs. It is possible, therefore, that the reduction of the Past Participle ending represents part of another sound change, namely the reduction of unstressed vowels in general.

3. Discussion

3.1. It is clear from the data presented above that the scribes of the Titian MS show systematic differences in their speech (and writing) habits. Depending on the features under consideration, the scribes can be related to each other in series like those in (13 A) or (13 B).

(13 A) α ε β ζ δ γ cf. (2), (8), (9), (12)
(13 B) γ δ ζ β ε α cf. (3), (4), (5), (7), (11)

In either case, scribes $T\alpha$ and $T\gamma$ represent extreme (positive or negative) deviations from the norm represented by scribes $T\beta$ and $T\zeta$ (note that $T\zeta$ was also the corrector). In general, the scribes seem to fall into two relatively well-defined groups, namely $T\alpha\epsilon\beta$ on the one hand and $T\gamma\delta\zeta$ on the other.

An adequate description of the dialect of the Tatian MS must account for these facts. It is obvious, then, that the description contains in addition to, or instead of, the usual categorial rules as in (14), at least some rules which define linguistic variables in the sense of Weinreich, Labov, and Herzog as in (15).

(14) $A \longrightarrow B \;/\; X \underline{\quad} Y$

(15) $A \longrightarrow g(B) \;/\; X \underline{\quad} Y$

$g(B) = f\,(C,\ D,\ E\ldots)$

where C, D, E ... are linguistic or extralinguistic variables. The remainder of this discussion is an attempt to determine these extralinguistic variables.

3.2. Although the Taian scribes seem to fall into two recognizable groups, we can eliminate the factor of sex since all of them were (presumably) monks in the monastery at Fulda. We may also disregard the factor of socio-economic class since all monks were equal, at least in theory and in the eyes of God; all scribes probably belonged to the same rank in the monastic hierarchy. It is, of course, conceivable that the socio-economic class of a monk's parents determined, in part, his rank or status in the monastery. We may further disregard the factor of education since anyone able to read and write both Latin and German in the early part of the 9th century must have received the same basic education in a monastery school. Of course, portions of the Tatian MS show recognizable differences in the rendition of Latin into more or less idiomatic German, but these differences must be attributed to the translators rather than the scribes.

This leaves the factor of age for further investigation since the scribes were ranked in (13 A) and (13 B) above on the basis of the time-line established by the Monastery Records and the other Fulda documents. However, this correlation gives us only the approximate age of a given scribe in the sense that it corresponds to the date at which he entered the monastery, received his training and became a scribe, which in turn corresponds to his real age. This factor cannot be eliminated by general considerations since the monastery at Fulda had existed for at least 75 years and several generations of scribes before the Tatian MS was written. However, the age factor as well as the other factors mentioned above must be eliminated because they define a linguistic variable as a single-valued function in the sense that a given individual at a given time must belong to one sex, one socio-economic class, one educational level, one age. But unless we disregard an entire subset of the data or change our interpretation of it -- and there seem to be no compelling reasons for doing so -- we have to accept the fact that the linguistic variables in this case are defined by a two-valued function. In this sense, a given individual must be characterized by a social feature such that the actual value of the function which defines one linguistic variable is (relatively) low while the actual value of the function which defines a second variable is (relatively) high. In fact, this factor must account for the actual reversal of the values for all scribes depending on whether the linguistic variable belongs to group (A) or group (B). Of course, one would have to know what distinguishes these two groups of variables from each other.

3.3. Despite the outward appearance of uniformity in its written records, maintained in the monastery school by tradition and monastic discipline, a monastery is not a homogeneous or even a normal heterogeneous speech community. Church rule and monastic recruitment practices affect the composition of the community at any time. The monastery dialect is thus a composite of the native dialects of its past and present members. If there is a "norm" for such a dialect -- and the evidence of the corrector's work argues for the existence of a norm at Fulda when the Tatian MS was written -- then the norm will incorporate features from a variety of existing dialects. In the case of Fulda, we have a monastery established on Middle German ground by Anglo-Saxon missionaries. The first generations of native monks came primarily from the Upper German dialect area in the South. As a consequence of successful missionary activity, later generations of monks began to come in increasing numbers from the surrounding Middle German dialect area and, in some cases, apparently even from the Low German dialect area in the North. The dialect norm at Fulda in the early half of the ninth century is basically East Franconian (the same dialect which later formed the basis of Modern Standard German). Those features of the Fulda dialect which were derived from Upper German sources may be marked as "positive," those derived from Middle and Low German sources as "negative." The linguistic variables which establish the ranking of scribes in (13 A) turn out to be positive in this sense, those which establish the ranking in (13 B), negative.

It seems that under these circumstances the place and dialect of origin of the individual scribe becomes the over-riding socio-linguistic variable. The distance between Fulda and a place of origin to the South of it may be expressed by a positive value, while the distance between Fulda and a place of origin to the North of it may be expressed by a negative value. Since Fulda would have a fixed value on the North-to-South scale, a given scribe's distance value is added to that of Fulda for a "positive" feature, subtracted from it for a "negative" feature. The distance values for scribes $T\zeta\delta\gamma$ are positive and increasing in this order; they are negative for scribes $T\beta\epsilon\alpha$ and increasing in this order. The actual value of the function defining a "positive" linguistic variable will then be largest for scribe $T\gamma$ and smallest for scribe $T\alpha$ (cf. (13 A)), while the actual value of the function defining a "negative" linguistic variable will be largest for scribe $T\alpha$ and smallest for scribe $T\gamma$ (cf. (13 B)).[8] The distance values for scribes $T\zeta$ and $T\beta$ are so small that they do not seem to add or subtract much from the standard value for Fulda.

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[8] All the data presented in section 2. above seem to support the assumption that scribe $T\gamma$'s place of origin was considerably further to the South of Fulda than scribe $T\alpha$'s place of origin to the North of it. This assumption may help to explain also the large number of corrections per page in $T\gamma$ compared to that of the other scribes (cf. (1)).

We can, therefore, replace the general formulation (15) of the linguistic variables under discussion by a specific statement such as

$$(16)\quad g(B) = f\,(L_F \pm k_i \;\; d_j)$$

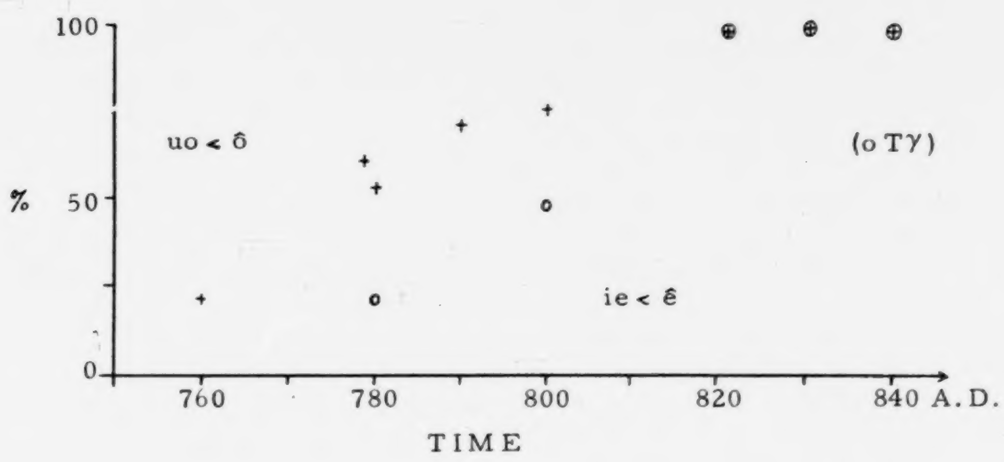
where L_F represents the location of Fulda, d_j the distance between Fulda and the place of origin of scribe j, and $\pm k_i$ a constant for the linguistic feature i and its source.

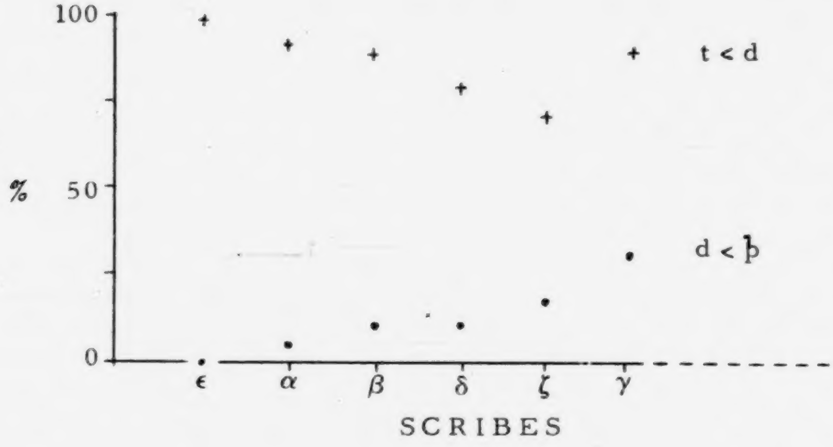
4. Summary

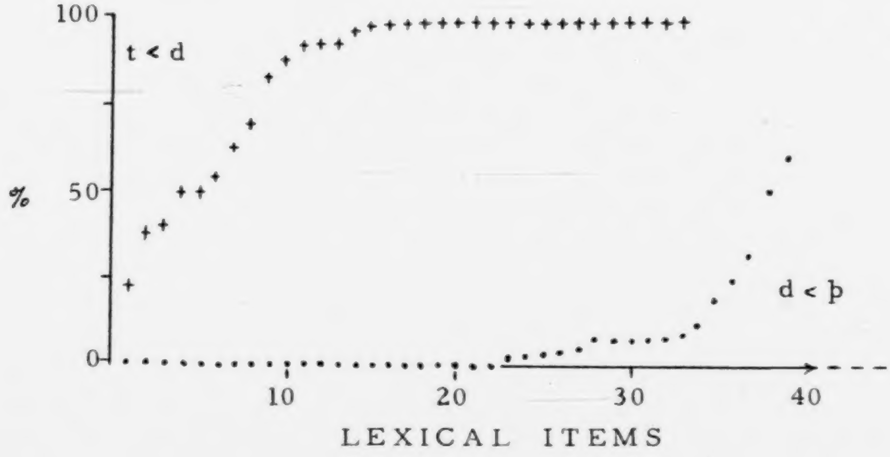
The present study demonstrates that a limited sociolinguistic analysis of historical documents may be feasible under certain conditions. The determining factor in the case of the OHG text under discussion turned out to be one which represents the distance between an individual's place of origin and his place of residence. This factor may override such well-known sociolinguistic variables as age, sex, education, and socio-economic status; it is obviously relevant for the analysis of speech patterns in societies characterized by a high degree of individual mobility. It seems to me that the concepts developed in this study allow us to clarify and to formulate the traditional notions of "dialect mixture" and "mixed language" in historical as well as in descriptive perspective.

APPENDIX

Graphical Representations of Sound Change Trajectories







MODIFICATIONS IN THE SPEECH OF AN EIGHT-YEAR-OLD AS A REFLECTION OF AGE OF LISTENER

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Introduction

With the appearance of Syntactic Structures in 1957, emphasis on the notion of linguistic competence and the intimate relationship between language acquisition and linguistic theory gave new impetus and direction to the study of child language. Confronted with the overwhelming observation that the child masters the essentials of his native language by a very early age (McNeill(1966) suggested three-and-a-half years), a number of theorists turned their attention to the innateness and universality of language ability. In so doing, they most often ignored -- or even denied the importance of -- the child's linguistic environment, which was considered to be random, haphazard, and even largely ungrammatical (see Chomsky, 1967 and McNeill, 1966). Within the past few years, however, there has been increasing discomfort with this disregard for a very real fact: as any interested observer can tell you, adults simply do not speak to young children in the same way that they speak to adults. Furthermore, as Broen (1973) has noted, even if the child has innate linguistic skills, i.e. LAD, which allows him to construct and test hypotheses about his language, he must still discover the linguistic forms that are peculiar to this language, and the information for his analysis must somehow be embedded in the speech he hears.

In the light of such observations, a number of recent investigators suggest that adults actually modify their speech considerably when speaking to children, and have begun to try to characterize the linguistic input to the young child and make hypotheses about its role in the child's language development (as discussed by Farwell, 1973). For example, studies by Drach (1969), Snow (1972), Phillips (1970), Broen (1973), and Remick (1972) have quantitatively described and contrasted the speech of adults to other adults and the speech of adults to young children. In general, these studies indicate that there is systematic variation between these two varieties of speech along the parameters of rate and fluency, syntactic and semantic complexity, and prosodic features. Adults' speech to young children, which we can classify as a distinctive register, appears to be slower and more fluent, less complex syntactically and semantically, higher in pitch, and more varied in intonation than their speech to adults.

The current thinking among some linguists, such as Labov, Hymes and George and Robin Lakoff, is that the individual's ability to shift styles is part of his linguistic competence. On the basis of this notion and the results of the above-mentioned studies, the present investigation hypothesizes that children who have essentially acquired competence in their native language will similarly modify their speech to young children. A second hypothesis, based on Phillip's work, is that these modifications will vary as a function of the age and linguistic competence of the listener. A recent study by Shatz and Gelman (in press), which came to our attention only after the completion of our own work, also supports this hypothesis.

In spite of the recent interest in the young child's language environment, few attempts have been made to describe children's speech to other children. Two exceptions are the above-mentioned paper by Shatz and Gelman, and an observational study report by Gleason (1971) concerning code switching in children's language. The Gleason report concludes that children -- specifically four-year-olds and eight-year-olds -- do indeed talk in different ways to different people, with some of their modifications reflecting adult registers and some not. In their work, Shatz and Gelman investigated the speech of four-year-olds to adults, peers, and two-year-olds, along more limited parameters. They concluded from the combined results of three separate studies that the four-year-old adjusts his speech with regard to the changing capacities of different aged listeners.

Synthesizing the results of the investigations of adult-child and child-child language varieties, our study made the following specific predictions: when speaking to a younger child, an eight-year-old will speak more slowly than to an older child or to an adult; similarly, syntactic boundaries will be better marked and there will be fewer disfluencies. Along the syntactic dimension, the mean length of utterance should be shorter, and this will reflect specific syntactic modifications, such as: fewer complex sentences; more instances of missing function words, inflections and copula in environments where they are required; and more one-word utterances. Semantically, speech to a young child should reveal a lower type-token ratio (that is, fewer distinctly named concepts in a sample of a given limit) and also more repetition. Along the phonological parameter, pitch will be higher and the intonation contour more varied when a younger child is being spoken to. In speech to a very young child, we would also expect to find some modification of phonological segments.

Method

In order to investigate these hypotheses, the present study examines the speech of an eight-year-old girl to an adult, to a peer, and to younger children aged five, three and one-and-a-half years. The peer and three-year-old were girls, the five and one-and-a-half year olds boys. All of them were well acquainted with, but not related to, the subject. The subject was recorded interacting with each of the addressees in three different linguistic environments: story telling, explanation of a block-stringing task, and free play or conversation. The first two situations were designed to provide comparable speech samples across addressees. All three conditions intentionally reflect the methodology of, and thus allow comparison with, the various studiès of adults' speech to children.

In the story situation, the subject told her own version of Little Red Ridinghood, using the pictures from an illustrated French translation of the story (which she could not read) for her own cues, and as an interest-holding device for her listeners. For the block-stringing task, the subject was given a set of different colored plastic triangles, squares and circles, and asked to string them together in a pattern of her own design. She then wrote down her pattern and told each of the subjects how to duplicate it. While the third situation was constrained only by the availability of a number of toys, it took the form of conversation about swimming and school with both the adult and the peer, and turned into game-playing with the three younger addressees. The order was mixed -- though not truly randomized -- so that no one subject or condition (with one exception) was recorded more than twice consecutively. A Sony TC-800 B tape recorder with a directional microphone was used to record the sessions in the two experimenters' homes over a period of two weeks.

Each speech sample was independently transcribed and analyzed in terms of previously defined measures by the two researchers. These analyses are being compared for percent agreement to provide some measure of reliability. While it is not possible here to provide a full explication of the criteria used for our measures, some examples of the decisions that had to be made are included in the following discussion of the measures. As far as possible, our decisions were based on the precedents set by the other studies.

Rate was measured in words per minute, based on the number of words in the first minute of fluent speech in each sample (see Brown, Salerno and Saches, 1972). All full words, but no word fragments were included.

Disfluencies included empty hesitations, which we define as all pauses which occur word- or sentence-medially (such as Little Red Riding ... hood), filled hesitations (such as um), false starts and retraced constituents. To get comparable figures for the various samples, these were counted, then calculated on the basis of disfluencies per one hundred words. The number of run-ons was counted to determine whether utterances to the younger children were potentially easier to isolate.

Mean length of utterance was calculated on the basis of words per utterance. While it is always difficult to define "word" and "utterance," this measure is found in other studies, Snow and Phillips, for example. Other researchers, such as Remick, have attempted to avoid this problem by counting morphemes and clauses. For our purposes, "word" was defined as a standard orthographic unit. Proper names (such as Little Red Ridinghood), contractions (such as I'll, you're and can't) and exclamatory expressions (such as uh-oh) were counted as one word, while the concatenated forms gonna and wanta and hafta were counted as two words. Based on Phillips, an "utterance" was defined as the conversational equivalent of a sentence. Intonation contour, pauses and grammatical independence were the criteria used to isolate utterances, although these did not always occur together.

Type-token ratio was determined by dividing the number of types by the number of tokens in each sample. Each different word was counted as a "type," and each occurrence of a word was counted as a "token." Different forms of the same word (such as *am*, *is* and *were*) were counted as one type. For the freeplay situation, each sample was limited to the same number of utterances to allow comparison. Although the story and the block-stringing task consisted of different numbers of utterances for each addressee, the whole of each sample was counted for this measure, as a comparable amount of conceptual ground was covered within each condition, and none of the samples were large enough to exhibit the general fact of language, that the larger the speech sample, the lower the TTR.

Repetitions, following Snow's procedure, included full, partial and semantic repetitions, counted separately. A full repetition is simply an exact repetition of the preceding utterance. An utterance was counted as a partial repetition when at least a phrase from the preceding utterance was repeated. A semantic repetition is defined as a paraphrase of the preceding utterance. In a few instances, an attention-getting utterance or word, usually the listener's name, intervened between a sentence and another utterance which was counted as a repetition.

In gathering acoustic data, a Kay type B/65 sonograph was used for narrow-band spectrographic analysis. Such an analysis allows for the calculation of the harmonics of the fundamental frequency at any given moment, as well as the calculation of the fundamental frequency itself. Spectrographic analysis was made on recordings of five comparable utterances of story narration, five comparable utterances of story role-playing, and two control utterances of freeplay speech to the adult. The spectrographic display was adjusted to exhibit the output of a 45 Hertz band-pass filter in the frequency range from 40 to 4000 cycles per second, with an automatic gain control of 1.5. Base fundamental frequency for each utterance was computed by averaging seven points through relatively flat and largely comparable sections in each spectrographic display of a set.

Results and Discussion

In general, the preliminary results of this pilot study do confirm our hypotheses. For mean length of utterance (MLU), Figure IA (next page) illustrates that with two slight exceptions the MLU decreases progressively with the lower age of the person addressed in each task. The first exception, in the block-stringing task, might be anticipated by the simplicity of the task, which therefore required little explanation by the subject; and the related observation that in this situation the subject (here after K) tended to collapse instructions to the adult and the peer (e.g. "And now two triangles") but not to others. This phenomenon is further reflected in the number of utterances it took to explain the task; in descending order of age of listener, the number of utterances were: 14 (to adult); 28 (to peer); 37 (to 5-year-old); 34 (to 3-year-old); and 125 (to 1½-year-old). Although discussion of sentence types is not included here, we assume that MLU is an accurate indicator of syntactic complexity, as argued by Wang and others (for discussion of this, see Wang, 1969).

Figure I

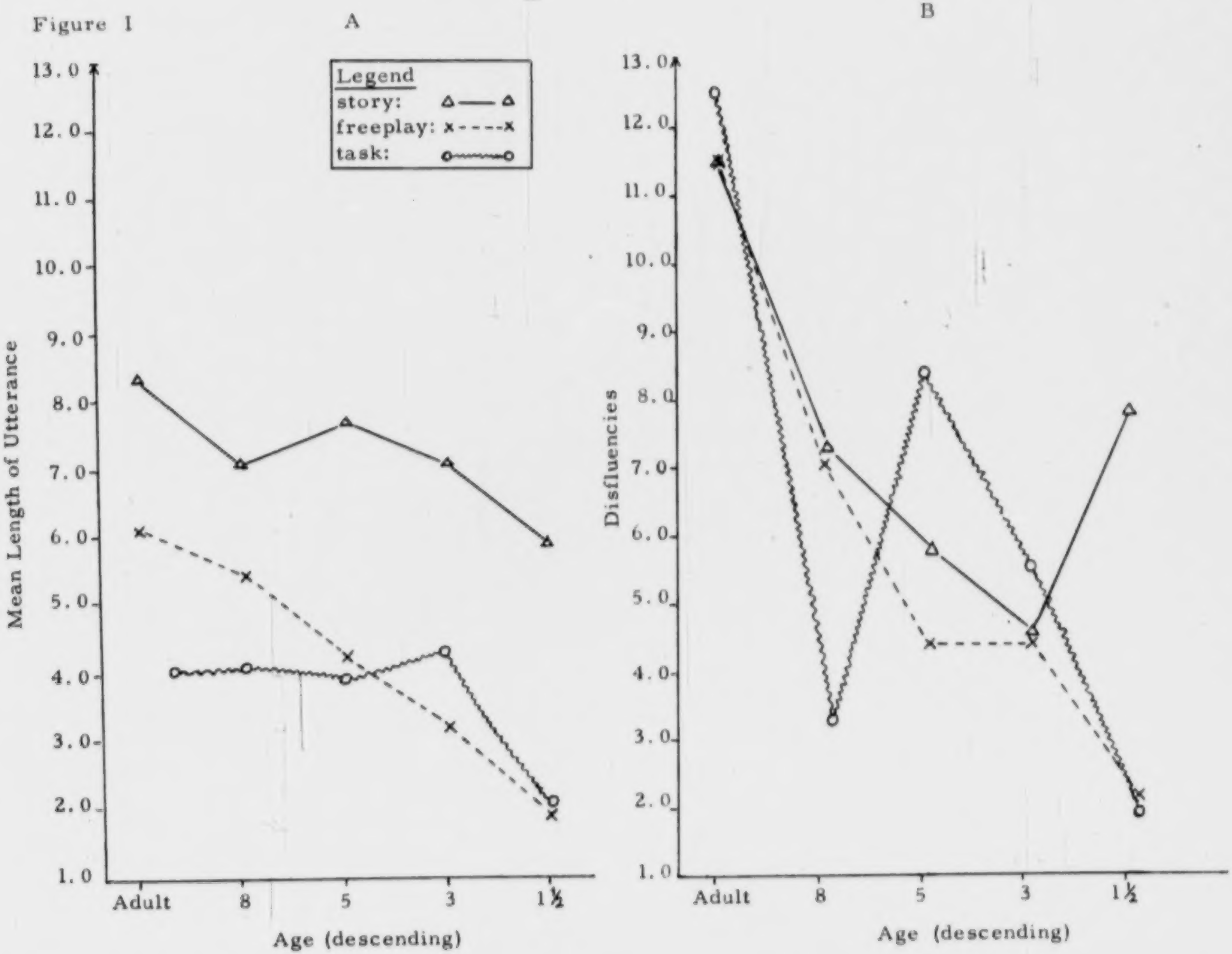

A
B
Legend
story: Δ——Δ
freeplay: x----x
task: o~~~~o
Mean Length of Utterance
13.0
12.0
11.0
10.0
9.0
8.0
7.0
6.0
5.0
4.0
3.0
2.0
1.0
Adult
8
5
3
1½
Age (descending)
Disfluencies
13.0
12.0
11.0
10.0
9.0
8.0
7.0
6.0
5.0
4.0
3.0
2.0
1.0
Adult
8
5
3
1½
Age (descending)

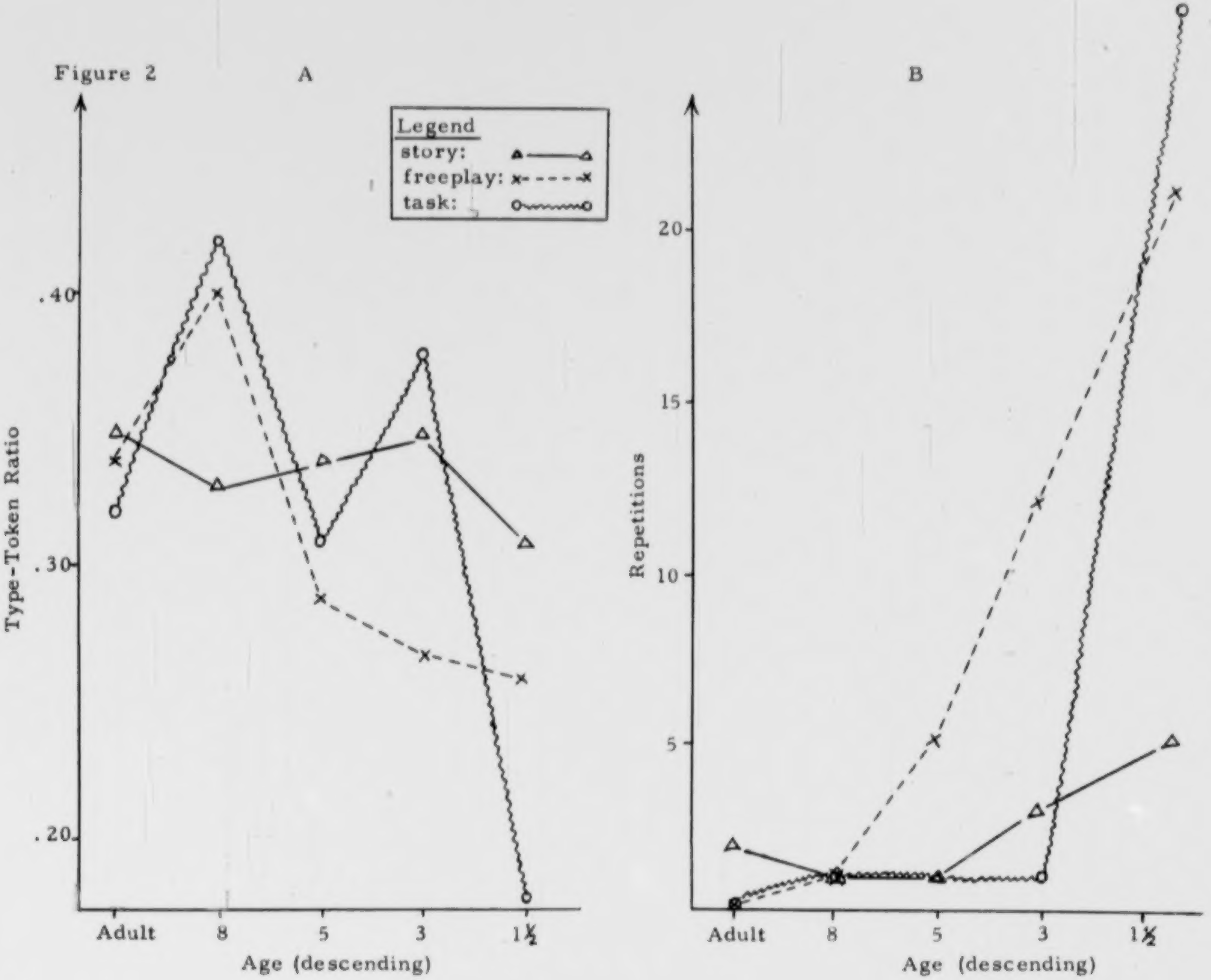
Figure I B shows that disfluencies do decrease as hypothesized, except in the block-stringing task (for the same reason of over-simplicity) and in one case in the story where the number of disfluencies to the youngest child is much higher than expected. This exception, though, could be attributed to the subject's frustration due to the inattentive behavior of the listener, who was too young to sit quietly through the whole story and therefore escaped into play halfway through this task.

The type-token ratio analysis shown in Figure 2 A reveals that in the story telling, there is an almost negligible difference in the range of vocabulary, whereas in the task and free play the greatly limited variety of vocabulary to the youngest child is quite marked. Interestingly, in both these environments, the variety of vocabulary was greatest in speech addressed to the eight-year-old. This, we believe, is a factor of the peer register, noted by Gleason, which is characterized by "rich use of expressive words like yukk and blech"(and in K's case also guy and gross) and also by rich use of sound-effects -- K's knock-knock and cluck.

The total number of repetitions -- including partial, complete, and paraphrase -- is given in Figure 2 B. The results are as predicted, with the added factor that the small number of repetitions to the oldest addressees were largely paraphrases or partial repetitions, compared to a larger number of complete repetitions to the youngest; this could, perhaps, be regarded as a reflection of an acknowledgement on the subject's part that the young children needed more time to understand.

As would be expected, there are cases in the speech to the three youngest children where required inflections and function words are missing. However, the number of such instances is very low. In free play with the one-and-a-half-year-old (while in the majority of possible places of occurrence these are absent) the total number if slight because the corpus consists mostly of one- or two-word utterances. With a larger corpus, we might have been able to exclude one-word utterances. Or, perhaps if the addressee had been a two-year-old, we could have avoided this problem by obtaining a corpus with more lengthy utterances.

Figure 2


A
Legend
story:
freeplay:
task:
Type-Token Ratio
.40
.30
.20
Adult
8
5
3
1½
Age (descending)
B
Repetitions
20
15
10
5
Adult
8
5
3
1½
Age (descending)

Figures 3 and 4 are concerned with prosodic variation with age of addressee. Figure 3 A shows an analysis of fundamental frequencies, while 3 B gives examples of spectographic traces of five representative utterances. Figures 4 A and 4 B illustrate ranges of pitch in story narration and story "role-playing," respectively. In our preliminary analysis of prosodic features, we found quantitatively that within story narration pitch rises (3 A) -- and range and deviation from the base fundamental frequency increases (4 A) -- as a function of the younger age of addressee.

Moreover, as expressed in panel B of table 4, we noted that role-playing within the story -- for example taking the part of Little Red Riding-hood -- involved raising of pitch (even over that of straight story narration) and a generally more varied intonation contour, but that the correlation with age of listener disappeared.

Also, more observationally, we noted that pitch is higher -- and intonation contour probably more exaggerated -- in story narration than in non-emphatic free conversation; these differences are illustrated by the tracings of sonographs in 3 B. Unfortunately, the only freeplay situation we've thus far been able to quantify this for is the adult sample (in 4 A), which we've therefore used as a base line comparison.

All this, then, led us to an unanticipated but interesting finding: the material in all the graphs -- and in additional data not presented here -- suggests the possibility that there exists -- for this eight-year-old, at least -- a distinct story-telling register, which tends to level out the differences which are elsewhere directly correlated with age of addressee. The modifications within this register seem to be more often in the direction of baby-talk style with, for instance, words per minute less in the story than in free-play to an adult, and generally greater variation in pitch. Sometimes this direction does not hold; however, as in the case of Mean Length of Utterance which increases across addressees. It is interesting to note that the modifications least leveled in this register -- in a sense, the "last to go" -- are phonological aspects and disfluencies.

A few discrepancies we found seem to stem at least partly from methodological questions. One of these concerns difficulty of task; the block-stringing task we chose was so simple for the adult and older children that K assumed knowledge on their part and did not bother to be explicit. In addition, the experimenters did not specifically request the subject to give a general task description -- i.e. her version of "You have to put the blocks on the thread in a pattern which I'll describe to you." Another problem is the ordering of the sessions. For instance, the order of story-telling may have affected the fluency and rate to the adult (who was first, this fact causing more disfluencies and fewer words per minute). However, with only one subject this is hard to avoid: somebody has to be first and somebody has to be last.

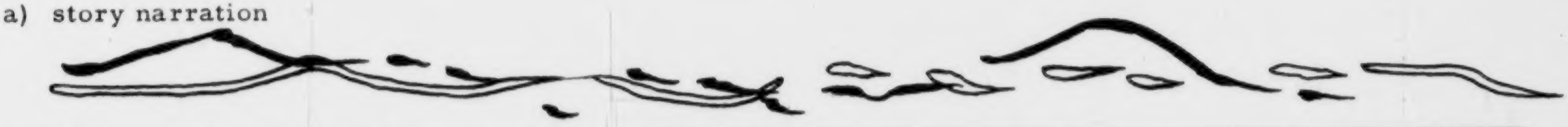
Figure 3 A: Spectrographic Analysis

Difference in Base Fundamental Frequencies

	Age: Adult	8	5	3	1½
a) story narration	329	317	300	404	379
b) story role-playing	330	350	460	405	410
c) freeplay	{255 {240				

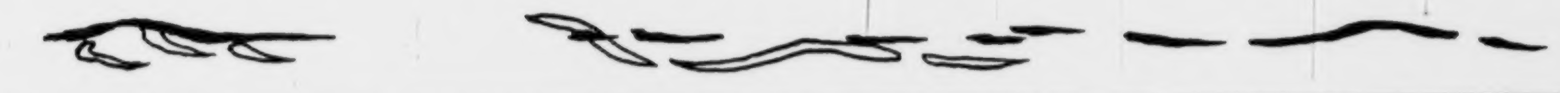
B: Examples of Spectrographic Traces

a) story narration



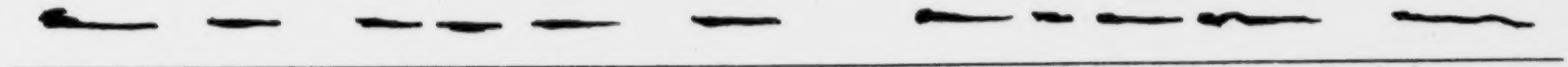
solid line (child): An' on the way, she stopped to pick some pretty flowers.
hollow line (adult): On Little Red Ridinghood's way, she stopped an' picked some flowers.

b) story role-playing



solid line (child): I'm bringing a basket of goodies to my grandmother's.
hollow line (adult): And she said, "To my grandmother's."

c) free play speech



(adult): Me an' my friends stayed inside, an'we were sl- sp- singing some songs, but in English.

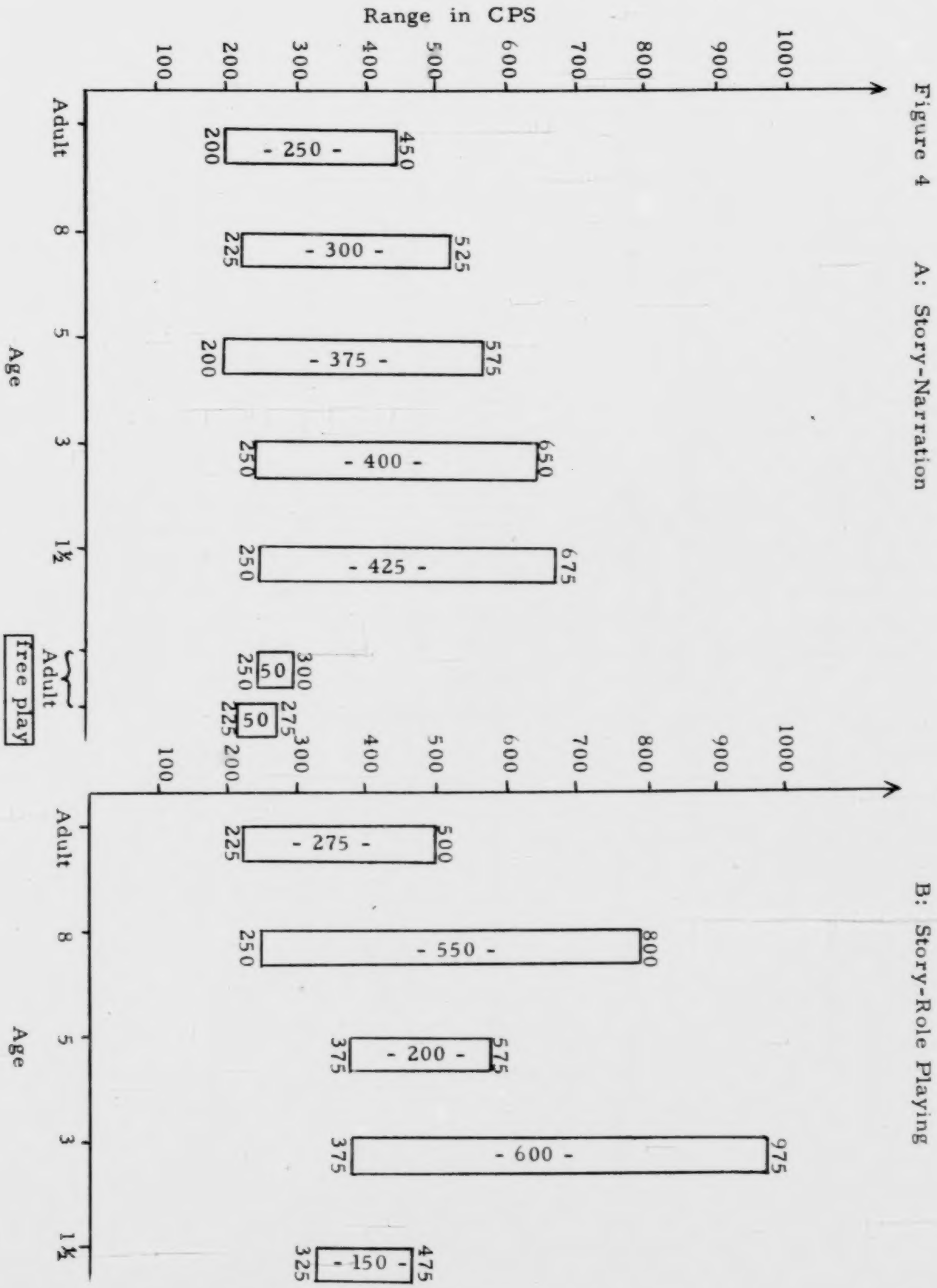


Figure 4

A: Story-Narration

B: Story-Role Playing

Before concluding, there remains one case of an exception which we earlier promised to discuss. If you recall, the five-year-old seems to be in a sort of "no man's land": sometimes modifications in his samples are more than expected, sometimes less. Sometimes he seems to group with the older two subjects, sometimes with the younger two. In other words, the subject seems to have ambivalent feelings about just how to react to him. We believe that this is caused by an interplay of three variables: sex, amount of verbal feedback, and age. For Type-Token Ratio and Mean Length of Utterance, the sex of the listener was probably an important factor. We postulate that perhaps the five-year-old's being a boy contributed to the closeness of the figures between him and the three-year-old girl (at least in free-play) since sex roles are very important to most eight-year-olds; and we hypothesize that if the subject had been conversing with a five-year-old girl, we would have found a greater Type-Token Ratio and a longer Mean Length of Utterance because of more interaction due to a greater range of shared interests. Closely tied to this (and perhaps primary) is the fact that K received little verbal feedback from the five-year-old, and therefore had few clues as to how to talk to him. The third variable — age — is of interest because Gleason says that children use basic features of Baby-talk Style to two-year-olds, but not to four-year-olds. This would suggest that the five-year-old should be classified with the adult and peer, but perhaps the break takes place slowly between the ages of three and six. This might explain the inconsistency here.

Which brings up one last point: when features of baby-talk register do appear in speech to the five-year-old (and to the three-year-old) they tend to cluster together in small sections of speech and to include grammatical changes (such as missing function words and inflections) along with both prosodic and segmental phonological modifications. For example: in free-play with the three-year-old, while the whole session contains only two instances of missing function words, within three consecutive utterances we get:

> You got bring two again.
> Oh, Kwistin, you got all two yours on.
> That's too bad, yeh?

and then, to the five-year-old:

> You have four more a go!

and a last example to the one-year-old:

> Want play game?
> Show me circu (i.e. circle).

In general, then, we may conclude that by the age of eight, the child seems to be in command of a variety of speech styles, with systematic modification based on such factors as age of listener and specific speech context (e.g. the posited story-telling register).

The present study represents a preliminary investigation of some of the social aspects of language acquisition and language use as a means of communication. While these results are based on one child only, they do reflect those trends observed in studies of adult modifications, and suggest that further, more detailed investigations are worth carrying out.

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